

Looking at the Bottom of the Cornucopia

FOR the best part of a century, the United States has been more concerned about the future supply of raw materials for industry than most other industrial countries. Anxiety of this kind at the turn of the century laid the foundations for the United States Administration's present intermittent involvement in the mining industry and for the policies by means of which the United States has been able to exploit the mineral wealth of North America at low cost and at an ever-quicker pace. Since the days of President Theodore Roosevelt, the mining industry in the United States has been dealt with, by taxation and other methods, more favourably than most governments treat even their farmers. Now, however, there is a chance that the United States government will be under pressure to take a tougher line with mining enterprises. In the past few years, several influential organizations have taken to saying that the chief objective of American policy on minerals should not be to ensure a cheap and plentiful supply but rather to conserve the resources which still survive in North America. The most recent contributor to the argument is the National Materials Advisory Board of the National Academy of Sciences which has just produced a thoughtful report on the subject (see page 424) which nevertheless does beg some important questions.

The starting point for the committee's argument is the now familiar lament that the annual consumption of the most important raw materials is still increasing rapidly. The United States itself is still the most prolific consumer even if the pace of growth is now quicker elsewhere, in Japan and even the more advanced of the developing countries. One result has been that the United States is no longer self-sufficient in a great many important raw materials—and it will be some time before the rowing dies down about the way in which chrome has found its way from Rhodesia to the United States in spite of the United Nations policy of sanctions. The National Materials Advisory Board, whose chairman is Professor Preston Cloud, is however more concerned with broader themes and in particular with the danger that some important raw materials will become exceedingly scarce within the coming century and with the social, economic and political effects of these developments. Thus while they argue that there is a continuing need for government sponsorship of exploration and for the development of new techniques in mining and extraction, and in the process make the important argument that there has been and remains a serious shortage of technical people with high qualifications in these fields, the essence of their case is that the United States government should consider whether the time has not come to abandon the policy of the encouragement of the supply of raw materials at low cost for the opposite policy of encouraging a restriction of demand. In particular, the board asks that the United States government should be more vigilant in monitoring the availability of raw materials, that no government policy or programme in any field should be adopted without an assessment of its effects on the depletion of

resources, that there should be a programme for the maintenance of strategic stocks, encouragement for recycling and other means of economy and that the government of the United States should feel free to rig the market and even to nationalize stocks of raw materials when the supply seems threatened.

It is therefore proper and perhaps necessary to understand that some of the recommendations of the Materials Science Board should not be taken seriously, on the grounds that their effects might be far different from what the good professors of geology no doubt intended.

Attempts to make a strategy for the conservation of raw materials are too often confused with other much broader questions such as the degree to which the present generation should order its affairs in such a way as to meet the needs of future generations. The question is often asked, in simple terms, whether it is right or even "moral" to consume raw materials now and, in the process, to deprive future generations of their use. Many of the recommendations of the Materials Science Board spring from similar considerations. The underlying error in this argument can be identified at several levels. For one thing, there is no knowing how future generations will decide priorities. If it should be that they value natural amenities more highly than the products which can be fashioned from mineral resources, they may in doing so implicitly settle for some degree of independence of the mining industry. It is also entirely conceivable that the pattern of manufacturing industry in future generations may be very different from what it is at present—indeed the experience of the past 150 years suggests that it will change in ways at present unpredictable. But if in future telecommunications, for example, dominates the industry as the manufacture of automobiles does at present, future advisory boards on raw materials will be worrying about quite different problems. And what if people take to playing chess at the weekends instead of travelling to the beach? In short, there is much in what the committee has to say for an attempt by central governments to relate public policies to the balance of supply and demand for raw materials, but the problem is rather one in sociology rather than in mining and metallurgy or even economics.

The belief that any present restriction of the present rate of consumption of raw materials must be of benefit for the future is also erroneous on economic grounds. The arguments of the conservationists usually lead to recommendations that would increase the true economic cost of extracting the materials on which present industry depends. In the present climate, when almost puritanical devotion to thrift seems to run through most arguments on natural resources, it is tempting to say that the extra

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economic cost is a necessary price for profligacy. The trouble is that such a point of view entirely overlooks the plain truth that the present comparatively low cost of raw materials of great economic importance contributes directly to the capacity of society to strengthen the social institutions on which the survival of future generations must surely depend. Those who think that it is virtuous to mine as little ore as possible, or to spend uneconomic sums of money on recycling, should ask themselves the question whether future generations will be better served by a little extra ore still to be recovered or by the development of, say, educational services (scientific research included) which, in their funny way, can be still more durable. And this, of course, is where the economic forces of the market are relevant as an arithmetical representation of the determination of social priorities. In a properly ordered society, to incur an extra cost of, say, £1 million now is to deprive society a hundred years from now of something in excess of £500 million of economic and social capital. Is this prudence or profligacy? May it not be much preferable that those who would make provision for the future in the exploitation of raw materials should spend their energies on making sure that the balance between supply and demand is accurately struck? There are plenty of targets to attack, like the protectionist devices behind which the United States petroleum industry shelters and the system of tax allowances which has made the price of metals in the United States uneconomically low (which is as much a folly as would be an artificial increase in price). Unhappily, as if dazzled by the bigger philosophical questions with which it has struggled, the Materials Science Board has nothing to say on this important issue—instead, it merely delivers the delphic opinion that the market is inadequate.

Creation in California

THE State Board of Education in California seems bent on an especially foolish course in its policy for approving science textbooks for use in the state schools. For the past three years, the board has been complaining that a new structure for the curriculum in California, and especially that at the junior high school level, allows no room for the teaching that the Earth was created, in the best biblical tradition. Fundamentalists on the board have argued that this is unfair, and that the teaching of the doctrine of the Creation should have equal place in the curriculum with the teaching of evolution. The result is that several large American publishers, conscious as they always are of the importance of a statewide adoption in California, have been busily modifying proposed textbooks along the lines suggested by the board. In one modified textbook, now a candidate for adoption in California, there appears the sentence "Many scientists think the Earth was formed with the rest of the solar system millions of years ago. Others believe in special creation of the universe, not nearly so long ago." Elsewhere (in another textbook by the same company) appears the statement "Scientists call this slow change to a new species evolution. Others do not accept the theory of evolution."

By all accounts, the issue of whether these textbooks should be used in Californian schools will be decided on

November 4. The issue is of course absurd. Even religious scientists no longer find it necessary to their position to deny the essence of the doctrine of evolution. And although there are many important questions about the details of the evolutionary process to be understood, especially at the molecular level, Darwinism occupies a place in science at least as strong as that of Newton's laws—no doubt there are many reinterpretations and refinements to come, but nobody in his senses can deny that the doctrine of evolution is an exceeding powerful means of relating such a variety of phenomena that it deserves to be called the truth, if in other than scientific circles, such a term is needed.

In all the circumstances, it is entirely disingenuous of the California State Board of Education to pretend to itself that sentences of the kind now being included in the textbooks submitted for approval are anything like a representation of the balance of considered opinion. Moreover, this is not a conflict in which a brave and embattled minority can reasonably be expected to restore Creation to the status of a scientific theory. To pretend otherwise is to misunderstand not merely the status of Darwinism but the nature of science. And who in any case is the minority? The State Board of Education in California has been indecently reticent on that subject. Who are the others? In the hope of helping to clarify the board of education's mind, *Nature* is prepared to send a free subscription to *Nature* to the first ten scientists working or teaching in a field of science bearing on the evolutionary question who are prepared to affirm that present observations are in their opinion inconsistent with the now commonly accepted views of Earth and species evolution. Applications must be received before October 30 and must give the present occupation of the applicants, who must be actively employed in the science department of a university. A list of names (if any) will be published.

100 Years Ago



THE British Association Meeting at Brighton has already begun to bear fruit in that town. A desire has been aroused among several of the inhabitants to know more of Natural Science, and a course of science lectures in the Dome, chiefly to working men, has been projected. But the ladies have taken the initiative, and the germ of a Ladies' Educational Association has already been planted. Prof. W. F. Barrett has been invited to give the first course of lectures on Experimental Physics. The introductory lecture on the "Study of Natural Knowledge," was given last Friday afternoon, when, in spite of the wet, upwards of 50 ladies assembled. Miss Goulty, of 2, Sussex Square, Brighton, to whom it is right to add the effort is mainly due, has permitted the use of her spacious schoolrooms for these lectures. The second lecture on "Magnetism" will be given to-morrow (Friday) afternoon.

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OLD WORLD

Threat to Deep Sea Research

FEARS are growing among oceanographers that a United Nations Conference on the Law of the Sea in 1973 could result in unprecedented restrictions on scientific research on the high seas. Proposals sponsored by a number of countries, notably the Latin Americans, would, if implemented, create an International Sea Bed Authority which would not only grant, or refuse, permission for scientists to conduct research in international waters, but would also supervise the research at all stages. All results and data would have to be approved by the authority before publication.

These crippling proposals are being strongly opposed by several nations—Britain, USSR and USA included—in the preparatory committee for the Conference on the Law of the Sea, but scientists are sufficiently concerned for the International Council of Scientific Unions to have passed a resolution in Helsinki last month demanding that open research in the oceans should be encouraged “and should not require consent when beyond the limits of territorial waters”.

The Latin American proposals are in fact only part of the role envisaged for the International Sea Bed Authority. The Law of the Sea has grown up in a comparatively piecemeal fashion since Grotius first framed international law in the sixteenth century. Jurisdiction over continental shelves was established by the Geneva Convention of 1958 which ruled that states have authority over their adjoining sea beds to a depth of 200 metres, or to whatever depth they are able to exploit the sea bed's resources—a definition that can at best be described as imprecise. Within the continental shelf areas lie territorial waters which vary in extent from three to twelve miles from land. Within territorial waters states have complete control and no foreign vessel may normally enter them without permission. Research may not be carried out except with permission of the relevant state, although it is usually granted (often after a wait of several months). Scientific research on continental shelves on, for example, waves and currents does not now require official permission outside territorial waters, but any research that involves the sea bed does require permission. Again this is usually granted. At present scientific research on the high seas is free of any restrictions.

In 1970, however, the UN decided to have a conference on the law of the sea, partly to tidy up international sea law and partly to provide for the none too distant day when the deep sea is exploited for mineral resources. American mining companies are already claiming that given a suitable legal climate they will be able by 1975, to recover copper and nickel from the manganese nodules dredged from the floor of the Pacific Ocean.

But the issues at stake in any revision of high sea law are clearly enormous. Huge economic and political considerations are involved, affecting mineral hydrocarbon resources, fishing rights, defence commitments, pollution, and unavoidably, scientific research. The preparatory committee which has been working on the conference in one form or another for the past five years, is the largest in the UN's history. More than ninety countries now hold a seat on it. The importance of the conference is highlighted by the appointment of Sir Roger Jackling, formerly Britain's ambassador in Bonn and a very senior Foreign Office official, as the head of the British delegation.

It has already become clear that some sort of International Sea Bed Authority is bound to emerge from the conference to regulate mineral exploitation; but the form it will take is far from clear.

The developing countries, with an eye on the mineral cake and only too aware that they do not have the advanced technologies necessary to exploit the deep sea, are anxious that the authority be an extremely powerful body, which will itself prospect and exploit the seas's resources on a commercial basis. The developed countries, aware that it is they who will have to provide the capital for such a venture, and worried about the overheads of such an international organization, not to mention the political difficulties of running a multi-national body with a multi-national staff, particularly when there is profit involved, are very much against so strong an authority. Most developed countries are willing, however, that an authority should be set up which would simply license states or companies to prospect, with some sort of machinery for assuring fair shares for all.

The scientific controls that the Latin American countries propose, spring from the same fear that they are losing out. They propose that international observers should accompany all research vessels on the high seas to ensure that the developing countries are in touch with the latest research, and they are unwilling to accept that all scientific results are openly published. There is a deep suspicion among the

ENVIRONMENT

Zuckerman Commission Attacked

THE Zuckerman Commission on Mining and the Environment has come under increasingly heavy attack from conservation bodies during the past week. In a letter to *The Times*, the heads of the National Trust, the Commons, Open Spaces and Footpaths Preservation Society, the Council for the Protection of Rural Wales, and the Standing Committee on National Parks have demanded that the commission's report should be “repudiated as an unbalanced argument for mining on a scale and of destructive effect not experienced hitherto in England and Wales”.

Earlier in the week the Standing Committee wrote to Mr Peter Walker, Secretary of State for the Environment, accusing the commission of “tendentially advancing the interests of non-ferrous metal mining at the expense of the environment”. The committee goes

on to say the commission's recommendations “put the mining industry into an unwarrantably favourable position in respect of planning control generally and encourage mining development in National Parks”.

The committee argues that the Zuckerman commission's recommendations for controlling exploratory drilling (see *Nature*, 239, 185; 1972) will in fact encourage the eventual granting of mining permission. Exploratory drilling involves capital investment, which “is always a strong argument in favour of development”, the committee argues. The committee also claims that the commission's twelve recommendations on pollution control and rehabilitation of the landscape “assumes a practicability of controlling pollution and of rehabilitation, which experience of existing extractive industries in England and Wales denies”.

developing countries that pure scientific research has, in fact, essentially economic aims. If all results were submitted to an International Sea Bed Authority the developing countries would be sure that they were not losing out.

Many oceanographers are horrified at the restrictions proposed. The restrictions placed on continental shelves are already irksome to some, and the delay in mounting expeditions and publishing findings that would result from the proposed UN agency's activities if they are extended to cover scientific affairs, are seen by many as deleterious to the progress of scientific knowledge about the oceans. In the words of the ICSU resolution "fundamental research by any nation carried on with the intent of open publication is in the interests of all".

The real fear is that when the conference finally meets — possibly in Santiago next year, although its deliberations will take many months—scientific research may be the loser in the inevitable trade off that will take place where the economic, political and defence interests of developing and developed nations collide. The extent to which political considerations may rule is indicated by the absence of any professional oceanographers from the delegations of most of the countries involved.

There is hope, however, in a middle road proposed by some of the more moderate developing nations, notably Sri Lanka (formerly Ceylon), which may result in complete freedom for scientific research with the exception of deep sea drilling which would be controlled by the International Sea Bed Authority in whatever form it finally takes.

NOBEL PRIZE

Antibodies Appreciated

THE 1972 Nobel Prize in medicine has been awarded jointly to Professor Rodney Porter, Whitley Professor of Biochemistry at the University of Oxford, and Professor Gerald Edelman of the Rockefeller University, New York.

The recognition of the work of these biologists on the determination of the structure of the antibody molecule comes twelve years after their epic studies were first published. At the time Professor Porter was on the staff of the National Institutes of Health Laboratories at Mill Hill, London and Dr Edelman was then, as now, at the Rockefeller University.

Professor Porter came to Oxford in 1967 to succeed Professor Sir Hans Krebs as head of the department of biochemistry. From 1949 to 1960 he



Professor R. R. Porter

was at Mill Hill and from 1960–67 he was Pfingst Professor of Immunology at St Mary's Hospital Medical School, University of London. Since 1970 Professor Porter has also been a member of the Medical Research Council.

When interviewed last week, Professor Porter said that he was delighted with the award which was completely unexpected but he would like to stress that he would not have been able to make such progress in understanding the structure of the antibody molecules but for Professor Edelman's complementary work. He knows Edelman well but has never worked with him.

Professor Porter is a product of the University of Liverpool and the University of Cambridge where he studied for his PhD under Dr F. Sanger, who himself received the Nobel Prize for Chemistry in 1958 for investigations which led to the establishment of the chemical structure of insulin. Porter's thesis work was concerned with the structure of antibodies and he has worked at intervals on this topic since.

Professor Porter is director of the Medical Research Council Immunochemistry Unit at Oxford which is concerned with the structural basis of both the specific affinity and the general properties of antibodies and also with the nature of cells involved in the immune response.

Erratum

In our report on the composition of the Australian Advisory Committee on Science and Technology (*Nature*, 239, 126; 1972), the following errors occurred: the chairman is Sir Colin Syme, not Mr Colin Lyme; Mr Alan W. Hamper should have read Mr Alan W. Hamer; Mr Russel T. Madigan's affiliation is Hamersley Holdings Limited.

The complementary work of Porter and Edelman culminated in a complete description of the structure of the human gamma globulin molecule. Porter, using rabbit anti-ovalbumin, was first able to divide the molecule into three functional parts: two identical fragments which bind antigen and must thus contain the sites involved in their recognition, and one fragment which is invariant.

Edelman then found a different way of splitting the antibody molecule by reducing it with mercaptoethanol in strong urea, showing that it was composed of polypeptide chains bound together by disulphide bonds. This led Porter to refine Edelman's technique and resulted in the confirmation of the four-chain model for gamma globulin: two heavy chains and two light chains, each with variable binding sites at one end and joined by disulphide bonds at the invariant regions at the other end.

Porter's technique made possible work in both laboratories on determining the amino-acid sequence of the variable portions of the heavy chains and showing that the variations were similar to those which had already been found in the light chains, suggesting that the two were complementary. The further development in Porter's laboratory of a method of splitting the heavy chain further, into more manageable fragments, finally enabled Edelman to sequence an entire immunoglobulin molecule for the first time.

Professors Porter and Edelman will share the award, which this year is worth in excess of £40,000.

RESEARCH AND DEVELOPMENT

Requirements Boards

THE Department of Trade and Industry has finally established the first of its long promised requirements boards. Mr Michael Heseltine, Minister for Aerospace, announced this week the names of the chairmen who will head five of the six requirements boards that are to be established, and the areas the boards will cover. They are ship and marine technology, chaired by Mr Nigel Brookes, Chairman of Trafalgar House Investments whose subsidiaries include Cunard and Offshore Marine; mechanical engineering and machine tools, chaired by Mr John Atwell, director of the Scottish based Weir Group of engineering companies, and vice-president of the Institution of Mechanical Engineers; engineering materials, chaired by Mr John Crane who is a director of Imperial Metal Industries Ltd; computer, systems and electronics chaired by Mr John Nichols, an under secretary at the DTI; and chemical and mineral processes and

plant chaired by Mr Donald Clark who is also an under secretary at the department. A standards and metrology board is also to be established, but the chairman has not yet been appointed.

The boards will take responsibility for the research and development programmes supported by the Department of Trade and Industry in its own laboratories, in industry and in universities, and for those research programmes that the department is to take over from the Natural Environment Research Council in April 1973. In effect they will implement the customer contractor principle within the department, and will spend between them about £15.6 million a year (see table).

The chairmen's first task will be to appoint the 12 members who will make up each board. These will consist of six independent members from industry, two independent scientists (usually from universities), and members from within the DTI, other government departments, the chief contractors (including the research councils), and staff from the DTI's research divisions. As soon as each board is complete it will survey the area of its responsibilities to assess the current direction of work and any changes that may be needed. The six chairmen of the board will form a separate committee which will meet two to three times a year to consider broad policy matters, and each board will publish its own annual report.

Mr Heseltine said that plans for separate boards for waste and effluent technology, safety and health and the laboratory of the government chemist had been dropped for the time being,

but he emphasized that "we are starting a new system (with the requirements boards) and if it doesn't work we'll divide them, or they can divide themselves."

Mr Heseltine also said that he was anxious that each board should have at its disposal, not only the necessary technical skills to deal with its subject, but also the necessary economic, marketing and public relations skills to take advantage of any successes.

On the question of conflict of interests when industrial members of the boards have to advise on research into areas that might affect their own businesses, Mr Heseltine said that whereas the problem was a difficult one, he hoped that the good sense of the board members and the activities of the DTI's staff would prevent difficulties occurring. It would be wrong, he implied, to cut off the advice of industry simply to establish a principle.

Expenditure on Research and Development within the Department of Trade and Industry

	£ million (approx.)
Civil Aerospace	109
Reactor and nuclear research	43
Industrial Research	
Establishments, Research	
Associations and contract research*	26
Total	178

*£15.6 million of this is to be spent by the requirements boards; the remainder is for providing direct assistance to industry on full repayment terms.

The requirements boards were first made public seven months ago by Sir Robert Marshall, secretary for industry at the DTI, when he gave evidence before the Select Committee on Science and Technology. Mr Heseltine's only explanation for the delay is that the administration needed to set up a new concept of research and development management took longer than he anticipated.

MEDICINE

Trauma over Trauma

TODAY about 13,700 people will be injured sufficiently seriously in England and Wales to have to attend hospital. Nearly half a million people, occupying ten per cent of available beds, are admitted to hospital each year as a result of being injured. Yet less is known about the pathology of trauma than about many other aspects of ill health which cause far less mortality and morbidity.

This accusation of neglect comes from a working party of the Royal College of Pathologists which began work in 1967 and has just issued its

New Appointment

PROFESSOR LESLIE FOWDEN, University College, University of London, has been appointed as Director of the Rothamsted Experimental Station in succession to Sir Frederick Bawden who died in February.

Professor Fowden, who has been professor of plant chemistry at University College since 1964, is 47 and has been a fellow of the Royal Society since 1964. He will take up his new appointment in the spring of 1973.

report with a string of recommendations as to how the situation can be improved.

The Medical Research Council should consider setting up special units to study the pathology of trauma, the University Grants Committee should finance multidisciplinary departments of experimental pathology to concentrate on trauma, and the General Medical Council should encourage medical schools to give greater emphasis to the study of trauma at undergraduate level. A Combined Forces Institute of Traumatic Pathology should be established by the Ministry of Defence to provide a national focus for research and a number of major accident centres should also be designated as centres of research into the subject.

Education, the working party says, will have to begin at an early stage in the educational cycle as there are so few experts on trauma in Britain, and attempts should be made to improve the statistics of trauma and its complications. And just to prove that the Royal College of Pathologists is not attempting to put other people's houses in order while ignoring its own back yard, the working party recommends that a greater knowledge of the pathology of trauma should be required of candidates in its own examinations.

As a result of the working party's report, the college has already been in touch with the Registrar General's office to encourage it to improve its compilation of statistics on injuries, and with the British delegates to the World Health Organisation who will be revising the organization's definitions of types of accident over the next three years.

But for real progress to be made more money will have to be put into research. Dr H. B. Stoner, the working party's chairman, said last week that trauma costs Britain more than £1 million a day; "we suggest spending rather less than one per cent of that on research, and if we cannot afford to spend any more, we cannot, perhaps, afford to spend much less".

Royal Commission

SIR Brian Flowers, Chairman of the Science Research Council, has been appointed Chairman of the Royal Commission on Environmental Pollution in succession to Sir Eric Ashby on his retirement from office on February 20, 1973. Sir Brian will hold the appointment for three years.

Sir Brian Flowers, who has been chairman of the SRC since 1967, is due to retire from that post in October of next year, when he will become Rector of the Imperial College of Science and Technology, University of London. Sir Brian is a theoretical physicist by training, holding degrees from the Universities of Cambridge and Birmingham, and his former posts include those of Head of the Theoretical Physics Division at Harwell, Chief Research Scientist at Harwell, and Langworthy Professor of Physics at the University of Manchester.

NEW WORLD

Launch Facilities for Sale Worldwide

by our Washington Correspondent

THE White House announced last week that the United States is prepared to launch scientific and applications satellites for any country willing to purchase the launch facilities. The only constraints on this broad offer are that the satellites must be for peaceful purposes and that the launching must be consistent with US obligations under international agreements and arrangements, particularly the Intelsat agreement.

The announcement signals the extension to other countries of the agreement reached last year between the United States and the European Space Conference (see *Nature*, 234, 266; 1971). According to Dr Robert T. Webber, acting Director of the office of space affairs in the Department of State, "it reaffirms that agreement at the highest possible level". Apart from European countries, the chief beneficiaries of US willingness to launch foreign satellites are likely to be first Japan and later possibly India. And, because the offer also extends to international organizations, specialized agencies of the United Nations have a route into space if they ever want to get into the satellite business.

The effect of the agreement is, however, largely symbolic for NASA has already launched some 16 satellites for foreign countries, and there seemed every chance that it would continue to do so even without such a broad agreement. But for the European Space Conference, the announcement last year and its reaffirmation last week is important because it divorces the question of launcher availability from discussions about possible European participation in the post-Apollo programme.

Until last year's announcement, there was considerable controversy in the European scientific community about whether or not the United States would continue to launch European satellites if the European Space Conference decided that it wanted no part of the post-Apollo programme. But the European Space Conference is now putting together its reaction to NASA's invitation to join in the shuttle and related programmes with the knowledge that American launchers will be available to member countries whatever they decide on the shuttle offer.

As for Japan, the broad offer of US launch facilities is being welcomed for several reasons. Although the Japanese space agency is building Thor-Delta

rockets under licence with the United States government, the launcher will not be powerful enough to place into orbit a series of meteorological satellites that the Japanese Space Developments Agency wants to launch in 1976. The satellites will be part of the Global Atmospheric Research Program (GARP), and negotiations were well under way between the Japanese and US governments for launch facilities well before last week's announcement. The announcement may, however, help oil the negotiations.

Both the Japanese and Indian governments are also thinking of setting up domestic communications satellite networks, and US launchers may be required to place them into orbit. The United States would probably have no objection to launching such satellites since they would be used solely for

domestic broadcasting and therefore would not come under the Intelsat agreement. As far as the Japanese system is concerned, the Thor-Delta rocket may be sufficiently powerful for putting the satellites into orbit, but it may not be ready in time. The Indian system, on the other hand, is unlikely to be ready until at least the late 1970s.

In short, the offer announced last week—referred to by President Nixon as "a global launch assurance policy"—is that the United States will provide launch facilities for other countries on a reimbursable basis. The launch could take place either from US sites or from foreign sites simply by purchase of a rocket. In theory, the offer also extends to China—Dr Webber said last week that such a prospect "may seem remote at the moment, but in seven or eight years time . . . ?"

RAW MATERIALS

A Limit to Growth

by our Washington Correspondent

AN *ad hoc* committee of the National Research Council has added its voice to the call for tighter government controls over the way raw materials are used in the United States. The committee, which met under the chairmanship of Dr Preston Cloud of the University of California, believes that unless a national materials policy is soon adopted in the United States, "one can foresee, within decades . . . the erosion of the United States mining, smelting, refining, and mineral-based industries, growing economic colonialism, international frictions, a steadily deteriorating balance of trade, and a tarnished global image of the nation".

The reason for the committee's dire prediction is the familiar story of burgeoning *per capita* consumption of natural resources, stagnant domestic production of raw materials and rapidly increasing world population. The committee points out, for example, that the United States is already almost completely dependent on foreign sources for twenty-two of the seventy-four minerals (coal and oil apart) considered essential for a modern industrial society, and cites a report published by the Secretary of the Interior earlier this year which estimated that by the end of the century, domestic supplies of primary raw materials would be sufficient to meet only 46 per cent of the demand. Unfortunately, however, the report, on the committee's own

admission, is more concerned with pointing out the problems than with providing the solutions.

According to Dr Cloud, however, there was little support in the committee for the view that market forces alone will solve the foreseeable problems related to better resource use, and the time is ripe for government action. A materials policy should be developed which should aim to re-examine the policies of material growth, base industrial production as far as possible on widespread and abundant basic commodities, more efficiently develop resources without "unacceptable" environmental costs and limit population growth.

How can the government help address these problems? One of the first requirements is a firm data base on which to build policies, and the committee therefore argues for more complete mineralogical mapping of the United States, more research on ecosystem dynamics and the development of more information on the trends in resource use. Having established the data base, the committee suggests that the government should provide "straightforward, equitable and flexible" incentives to help promote more efficient use of resources and to help base industrial production on the more abundant raw materials. Such government actions could include direct intervention in the market, either as buyer or seller, and even nationalization of limited stocks of critical materials. Specifically, the committee recommends that a permanent advisory committee on minerals policy should be formed.

Congress Clears the Decks

by our Washington Correspondent

ONCE again, Congress has provided its usual pre-election spectacular. Last week it passed more legislation than it would normally get through in a month—complex bills, conference reports and resolutions were often agreed to without debate and both houses even took the drastic step of meeting on Columbus Day and a Saturday. The reason, of course, is that the elected representatives of the American public were anxious to get home to campaign for re-election. Unless the bills had been cleared before Congress adjourned, the legislation would have been wiped off the slate. Among the measures which squeezed by last week were several items which involve science and technology. They include the following:

National Institute on Aging

Congress sent to President Nixon a bill designed to set up within the National Institutes of Health an institute for research on ageing. The fruit of recommendations made by the recent White House Conference on Aging, the institute will be concerned with behavioural research as well as biomedical research on the ageing process. No budget has been suggested for the new institute, which will start work in earnest in the fiscal year which begins next July, but it seems clear that it will take over some of the budget and the work of the National Institute for Child Health and Human Development (NICHD). Part of NICHD's brief is to conduct research on the ageing process.

The Director of NIH is empowered to decide which activities now being carried out by other institutes will be transferred to the National Institute on Aging—a formidable task since gerontological research could be defined to include a whole variety of activities. The idea is, however, that the new institute will not be concerned with research into chronic diseases which afflict the elderly, but that it will concentrate on the ageing process itself.

According to Senator Tom Eagleton, chief sponsor of the bill in the Senate, the reasons for setting up the institute are that little attention is now being paid to research on the ageing process; what research is taking place is fragmented and there is no single agency in the federal government for tying together all aspects of gerontological research. He also pointed out that the problems of the elderly are increasing, if only because the numbers of elderly Americans are increasing rapidly—

during this century, for example, the number of Americans more than 65 years old has increased seven-fold, and by the year 2000 it is estimated that there will be some 45 million in this age group, compared with the present figure of just over 20 million.

Ocean Dumping

Disagreements between the House of Representatives and the Senate over a bill to control the dumping of waste material into oceans and tidal estuaries have been resolved, and the measure was finally approved by Congress last week. Designed chiefly to prevent unregulated dumping of wastes into the oceans, the bill sets up a system of permits to be issued by the Environmental Protection Agency and the Army Corps of Engineers and provides for stiff penalties for violation. Also contained in the bill is an authorization for \$6 million to the National Oceanic and Atmospheric Administration (NOAA) to carry out research into the effects of ocean dumping and for global monitoring of ocean pollutants and other ocean problems. The House of Representatives had originally asked for \$2 million for this purpose, but the Senate requested \$12 million.

One of the chief bones of contention in the bill is a provision for setting up marine sanctuaries in oceans around the United States. The House bill contained the provision, which was strongly resisted by the Administration because the funding for the sanctuaries was open ended and because sensitive issues of international law are involved, but it was dropped from the Senate version. The conference committee reinstated the section, with a few modifications to get around the legal implications.

Marine Mammal Protection

A highly charged and emotional issue, which has occupied much of the time of the House Committee on Merchant Marine and Fisheries, and the Senate Commerce Committee during the 92nd Congress, was finally resolved last week. The issue is the protection of marine mammals, and Congress has decided to place a permanent moratorium on the taking of marine mammals and the import into the United States of products manufactured from them. There are, however, several specific exemptions from the moratorium and the Secretary of Commerce is given the power to waive it in some cases—a provision which some environmentalists consider seriously weakens the act.

In short, the bill provides that no marine mammal can be taken by US citizens or by others in US waters, and that no marine mammal products can be imported into the United States unless a specific permit has been issued by the Secretary of Interior or the Secretary of Commerce. Permits will be given to allow mammals to be taken for scientific research and display, by Eskimos, Indians and Aleuts who depend on them for a living and by tuna fishermen provided that every step is taken to try to prevent mammals from being caught along with the tuna. The most controversial part of the bill, however, is that which would allow marine mammals to be caught and their products imported on a regular basis, provided that such action would not harm the species or the stocks.

But before such permits are issued, a three-member Marine Mammals Commission, which is also set up by the act, must give its approval, and there must be provision for public hearings. The permits could also be revoked at any time.

Nixon on the Spot

Congress has sent President Nixon a political hot potato by again passing the Appropriations Bill for the Departments of Labor and Health, Education and Welfare, which he vetoed in August (see *Nature* 238, 428; 1972), with a rider which allows him to reduce the funding for any individual programme by 13 per cent. If Nixon agrees to sign the Bill he will therefore be forced to take the politically unpalatable decisions over which programmes to cut back and, as Mr Robert Michel said when the Bill was passed by the House of Representatives, "There could be so much backfire that the Administration would be hard put to make each and every reduction".

The Bill which Nixon vetoed would have entailed expenditures of more than \$1,800 million in excess of what he had requested, and he therefore refused to sign it because he considered it fiscally irresponsible. The Bill passed last week, however, allows him to reduce the expenditure to about \$500 million more than he had requested and Congress is hoping that he will find such a measure more palatable. The research institutes of the National Institutes of Health will be bound to receive more money than President Nixon had requested; even if Nixon applies the knife to the fullest extent that Congress has allowed, they will still end up with at least a 3 per cent increase over the budget request.

NEWS AND VIEWS

Plasma Tryptophan and Brain Function

THE indoleamine 5-hydroxytryptamine (5-HT) (serotonin) is thought to act as a neurotransmitter in the mammalian central nervous system. Although the precise role of this amine in brain function is not known, it has been shown to play an important part in regulating patterns of sleep in cats and other experimental animals. There is at present a great deal of interest in the possibility that some forms of depressive illness in man may be related to a deficiency of cerebral 5-HT (see Ashcroft *et al.*, *Lancet*, ii, 573; 1972), and it has been plausibly suggested that antidepressant drugs may act by enhancing the availability of 5-HT at synapses in the brain.

An understanding of the biochemical mechanisms involved in the regulation of 5-HT synthesis in the brain is thus of considerable importance. The article by Knott and Curzon on page 453 of this issue of *Nature* helps to add another missing link to the understanding of this regulatory process. There is now ample evidence that the rate of synthesis of 5-HT in the brain is regulated principally by the availability of the precursor amino-acid L-tryptophan, the normal concentration of which in the tissue is considerably below the K_m for the first enzyme in the 5-HT biosynthetic pathway, tryptophan hydroxylase (Eccleston *et al.*, *J. Neurochem.*, **12**, 493; 1965). Furthermore, Tagliamonte and his colleagues reported last year (*Nature New Biology*, **229**, 125; 1971) that various treatments and drugs which caused increased synthesis of brain 5-HT all led to significant increases in the concentration of tryptophan in the brain.

On the other hand, it has hitherto not been clear how the tryptophan pool in brain is controlled. Tagliamonte and his colleagues and Knott and Curzon had previously failed to establish any consistent correlation between brain and plasma tryptophan concentrations. For example, food deprivation or immobilization stress led to significant increases in brain tryptophan and 5-HT synthesis without any corresponding changes in plasma tryptophan. Nevertheless, brain tryptophan must presumably be derived from the plasma, for mammalian tissues are incapable of synthesizing this essential amino-acid. This paradox has now been resolved virtually simultaneously by Knott and Curzon and by the Italian group (Gessa, Biggio and Tagliamonte, *Fed. Proc.*, **31**, 2168; 1972). The solution lies in the fact that tryptophan is alone among circulating amino-acids in being highly bound to plasma proteins. Knott and Curzon have now repeated their experiments on food deprivation and immobilization stress in rats, and measured not only the total tryptophan content of plasma but also the much smaller ultrafiltrable fraction of free plasma amino-acid. They find that in rat plasma about 95 per cent of the total tryptophan is tightly bound to plasma macromolecules. The small free tryptophan fraction in plasma, however, showed changes which correlated well with changes in brain tryptophan.

After 24 h of food deprivation, for example, there was no significant change in total plasma tryptophan but very large increases in the free plasma amino-acid and in brain. Knott and Curzon also observed that there was

an increase in the plasma concentration of non-esterified fatty acids under conditions in which free plasma tryptophan was increased. Heparin, which is known to lead to an increase in plasma free fatty acid content, also caused an increase in the concentration of free tryptophan, suggesting that a casual relationship may exist between these parameters.

The findings of Knott and Curzon thus explain several hitherto paradoxical observations. As the authors point out, their findings suggest that it might be very interesting to measure the concentration of free tryptophan in plasma samples from depressed patients. These findings may also be relevant to improving the mode of administration of L-tryptophan, which is claimed to have useful antidepressant properties (Coppen *et al.*, *Lancet*, i, 1393; 1972).—From our Pharmacology Correspondent.

Outburst of Cyg X-3

THE radio outburst of Cyg X-3 early in September this year is described by one of the discoverers of the event, P. C. Gregory, as "the most impressive outburst ever witnessed by radio astronomers" (see page 439 of this issue of *Nature*). Certainly, radio astronomers and other observers have been so excited by this event that as the news spread everyone who had any chance of detecting the object, at any wavelength, seems to have directed instruments towards the constellation Cygnus. As a result of this unique focusing of astronomical attention on one object, more than a score of contributions probing the nature of Cyg X-3 have been gathered into a special issue of *Nature Physical Science* (next Monday, October 23) produced to coincide with the "discovery" articles of Gregory *et al.* (this issue, page 440) and Hjellming and Balick (this issue, page 443). But why are the astronomers so excited?

Perhaps the most important aspect of this event is that it demonstrates, for the first time, the existence of violently variable radio sources within our Galaxy. Cyg X-3 was being monitored because it is an X-ray source, and thus of particular interest in any case. But the outburst had no counterpart at X-ray frequencies and there is no reason to suppose that such radio outbursts should only occur in objects which are strong X-ray emitters; indeed, flares of this kind could have occurred many times in sources throughout the Galaxy and been completely missed by conventional surveys.

Basically, then, the excitement is the result of the discovery of what may be the archetype for a new category of highly energetic objects located within the Galaxy. The most violent events in which anything like

Additional copies of next Monday's *Nature Physical Science*, which contains reports of observations of Cyg X-3 at radio, infrared and X-ray frequencies, will be available (see page X).

a "normal" star participates are, of course, supernovae, which are believed to leave behind pulsar remnants. This "radio supernova" type of event could, perhaps, be related to other violent events, such as supernovae and the processes operating in pulsars. Perhaps the most significant development in astronomy in the past five years has been the realization that there is no need to look at distant galaxies and quasars to see violent events and processes which are producing large quantities of energy; first radio astronomy, with the discovery of pulsars, then X-ray astronomy, with the discoveries made by the satellite Uhuru (now joined by another X-ray experiment on the satellite Copernicus), have shown that violently energetic outbursts are not uncommon much closer to home. Cygnus X-3 has now shown that such outbursts are even less uncommon; one hope is that a better understanding of the violence which goes on in our Galaxy will help the understanding of the violent events seen in extragalactic objects.—By our Cosmology Correspondent.

ELEMENTARY PARTICLES

New Detector Hope

from a Correspondent

THE detection of transition radiation, which is emitted when a fast charged particle crosses the boundary between two dielectric media, is a difficult but increasingly promising technique for distinguishing relativistic charged particles.

The problem of telling particles—say pions and protons—apart becomes a complicated business at very high energies. Hence the birth of the new generation of synchrotrons at Batavia (Illinois) and CERN (Geneva), capable of accelerating protons to 300 GeV or so, calls for novel techniques of particle identification. The trouble is that the higher the energy of the particle the more closely its speed approaches that of light. Thus conventional methods (at 20 GeV), which often rely on variations in velocity, become more and more difficult to apply.

Relativistic charged particles are not, however, difficult to detect; they can leave a trail of ions in their wake and may therefore be easily manifested with spark or streamer chambers. The problem is identifying particles that have the same deflexion in a magnetic field and hence the same momentum.

For some years Luke Yuan of the Brookhaven National Laboratory (husband of the formidable Mrs C. S. Wu)

has been advocating transition radiation as a solution. It may be recalled that Čerenkov light is emitted if a particle passes through a dielectric medium at a speed greater than the velocity of light in the medium. In the case of transition radiation, photons can be given off when a charged particle goes from one dielectric medium into another. (It was the Russians Ghinzburg and Frank (in 1946) and Garibian (in 1960) who were responsible for the theory, and it would be appropriate therefore if this class of radiation could be named after them.)

From the point of view of particle physics, transition radiation has the advantageous property that the number of photons emitted is directly proportional to the particle energy, in fact to the Lorentz factor γ ($(1-\beta^2)^{-\frac{1}{2}}$ or E/m_0c^2). Furthermore, the emission is largely in the X-ray region. If, therefore, particles traverse a multilayered medium—a stack of foils or a piece of foam, for example—a count of the number of X-rays emitted should be a measurement of the energy of the particle.

Observing the X-rays, however, presents its own problems, partly because they are emitted essentially in the forward direction along the path of the particle. Sodium iodide or Ge(Li) detectors cannot sensibly be used in many instances because they are small and present a massive obstacle to the particle itself.

In their latest contribution, Yuan and his collaborators (*Phys. Lett.*, **40B**, 689; 1972) have considered the prospect offered by proportional counters with thin walls. This is not a simple option, for such a counter placed immediately behind the multilayered medium would

produce pulses proportional not only to the ionization of photoelectrons ejected by X-rays, but also to the simultaneous ionization created by the energetic particle itself in traversing the gas in the counter. The task is to subtract the latter component.

In a fixed length of path, the total specific ionization by a charged particle is described by the Landau distribution (*Russ. J. Phys.*, **8**, 201; 1944). This differs from the Poisson distribution in that it has a tail representing occasional large deposits of ionization. It is this tail which complicates the detection of X-rays, because the aim of the exercise is to see the X-ray contribution to the pulse height distribution above that due to the particle. With a single counter it is not possible to say whether a large pulse corresponds to X-rays or indicates a tail event.

Yuan *et al.* chose to eliminate the uncertainty statistically by using a cascade of counters—in fact a cascade of Charpak multiwire chambers (see Dimčovski *et al.*, *Nucl. Inst. Meth.*, **151**, 94; 1971). If, say, thirty chambers are interspaced with multilayered transition radiators thirty pulses will be obtained every time a fast particle traverses the array. By obtaining (electronically) the geometric mean of all the signals the effect of the low probability tail can be substantially eliminated, although one still has a broad distribution to contend with.

In some tests with 10 GeV electrons crossing the chambers, the authors were able to record large mean signals indicating the X-ray contribution. These are early days, however, and it remains to be seen whether physicists will plump for this potentially valuable technique for identifying particles.

Isolation of Slime Mould Messengers

To date one of the chief limitations to investigations of the control and mechanism of protein synthesis in eukaryotes has been a lack of adequate supplies of messenger RNA. The discovery that many eukaryotic messengers possess a 3' terminal stretch of 100 to 200 adenylic acid residues seems, however, to have provided biochemists with an opportunity to isolate messengers in large amounts. Firtel, Jacobson and Lodish, for example, in *Nature New Biology* next week (October 25) claim that it is now possible to obtain pure mRNAs in milligram amounts from the slime mould *Dictyostelium discoideum* by allowing extracts containing messengers to hybridize to immobilized poly(U); the poly(A) sequences in the messengers cause their specific retention.

The ultimate aim of Lodish and his colleagues is to programme cell-free systems with mRNAs obtained from

Dictyostelium cells at the several well demarcated stages of the life cycle of this organism and so discover the pattern of genetic switching. So far they have established that about 75 to 95 per cent of *Dictyostelium* messengers contain a run of about 100 adenylic acid residues, that about 85 per cent of these messengers are transcribed from DNA sequences which are present in the genome as single copies and that the messengers seem to survive undegraded the isolation procedures to which they are subjected.

As Lodish *et al.* comment *Dictyostelium* is an ideal organism for studying the control of gene expression, for there is ample evidence that control is exerted at both the transcriptional and translational levels. What can be said with certainty is that whatever else may crop up to impede their goal there seems to be no shortage of messenger RNA.

NUCLEIC ACIDS

Finer Structure

from our Molecular Biology Correspondent

THERE is evidently no end to the strange structural details in DNA that increasing technical ingenuity is capable of bringing to light. The most diverting example in some while is a closed circular DNA, which contains a ribonucleotide in one strand so that treatment with ribonuclease causes the supercoiled structure to untwist. The discovery has been made by Blair *et al.* (*Proc. US Nat. Acad. Sci.*, **69**, 2518 : 1972) in a colicinogenic factor, that is to say an extra chromosomal DNA, or plasmid, which occurs in *Escherichia coli*.

Previous work had shown that treatment with chloramphenicol leads to proliferation of the colicinogenic DNA, *col E₁*, whereas chromosomal DNA synthesis ceases after a short period, the number of molecules of *col E₁* continues to grow, and ultimately increases a hundredfold. The population generated in this way differs in a curious manner from normal *col E₁*. When examined by equilibrium density gradient sedimentation in alkaline calcium chloride, it bands predominantly at the buoyant density associated with linear strands. The scission of cyclized strands in alkaline solution was demonstrated by velocity sedimentation after incubation for various times in alkali. As lability to alkali is a characteristic of ribonucleotides as opposed to deoxyribonucleotides, the next step was to see whether ribonucleases had the same effect, and so indeed it proved. Pancreatic ribonuclease opened *col E₁* chains only from cells treated with chloramphenicol. Ribonuclease *T₁*, which is specific for guanine residues, did not work, but ribonuclease H did. This latter is a highly discriminating enzyme, the activity of which is confined to double stranded DNA-RNA hybrids. It will not operate on native or denatured DNA, or on single or double stranded RNA. Furthermore, when the open circular DNA, after treatment with pancreatic ribonuclease, was isolated and the strands separated in alkali, a one-to-one correspondence of circular and linear single strands was observed.

The inference is inescapable: after treatment with chloramphenicol the cells manufacture a supercoiled circular DNA, in which one strand contains one or a short run of ribonucleotides, paired with the deoxynucleotides in the complementary strand. Blair *et al.* do not neglect to leave the open-mouthed reader with a plausible explanation. They find that rifampicin, which is a specific inhibitor of RNA synthesis, stops the synthesis of normal *col E₁*, and also of the abnormal form after

treatment with chloramphenicol. Moreover in some other systems it has been reported that DNA replication is contingent on RNA synthesis. Perhaps therefore in these situations the DNA polymerase system will not function except in the presence of an RNA primer, which must be slipped into the existing DNA duplex and remains covalently integrated until the strand is complete, when it is finally eliminated by a specific strand scission and repair process. It can then be postulated that enzymes implicated in these final steps are absent in the cells treated with chloramphenicol.

A more familiar feature that has long been recognized in the linear DNA of bacteriophages is the repetition of terminal sequences. This takes the form of the presence of identical sequences, reading in the same direction, at the 5' and 3' ends of the same strand. The other strand of the duplex therefore likewise contains identical sequences at its two ends. In order to reveal the existence of this type of repetition it is only necessary to expose the DNA to an exonuclease specific for either the 5' or the 3' end. Working inwards on the two strands from opposite ends of the duplex it will leave single-strand tails, complementary to each other. These cohesive ends will base-pair under annealing conditions and cause cyclization. The latest demonstration of such a situation comes from Rhoades and Rhoades (*J. Mol. Biol.*, **69**, 187; 1972) and relates to T5 phage DNA. This differs from other T phages in containing a few natural single-strand breaks, one of which it now seems is in one of the terminal repeated segments, for if all breaks are first repaired with polynucleotide ligase, the degree of exonuclease digestion required to cause cyclization is substantially reduced. When the breaks are present, the exonuclease will cause liberation of what would be the cohesive end by eating away its complementary strand. More digestion will therefore be required to produce a new cohesive sequence. The DNA repaired with ligase, examined in the electron microscope after exhaustive exonuclease treatment and annealing, shows the internal double helices corresponding to the complementary tracts of the single strands so produced. These have a narrow length distribution, with 10,000 or so base pairs, or 9 per cent of the genome. If the DNA is not first repaired, the base paired sections are shorter, and variable in length. It seems likely that the natural break is not confined to a single location within the repeated sequence.

A new variation on terminal sequences has been unearthed in human adenovirus DNA by Garon, Berry and Rose (*Proc. US Nat. Acad. Sci.*, **69**,

2391 : 1972). In this case, cyclization does not follow exonuclease treatment. Instead denaturation and annealing, especially at low DNA concentration, lead to the formation of single-stranded circles. If the ends of the native molecule are eroded with a 3' exonuclease to the extent of 2 per cent or more, this transformation no longer occurs. This has been observed for all of a number of serotypes. It seems clear therefore that in this DNA the repetitive sequence occurs at opposite ends of complementary strands, so that there is self-complementarity within each. It remains to enquire whether the repeated sequences are read in the same or in opposite directions. In the one case annealing at the ends would produce a circle with a double stranded stalk, in the other a double helical segment as part of the circle. No structures with stalks are seen in the electron microscope. Denaturation on the other hand does expose a small loop of two separated strands in the circles, suggesting that the complementary sequences may be read inwards towards each other from the ends. Garon *et al.* report that the lengths of the repeated sequences—measured by the extent of exonuclease digestion that inhibits cyclization—correlate with the oncogenic activity of the serotype. They suggest that cyclization in the cell may be a step in the incorporation of the DNA into the host genome.

TECTONICS

Magmatic Differences

from our Geomagnetism Correspondent

As long ago as 1926, Benson (*Mem. 1*, **19**; National Academy of Sciences, 1926) found it convenient to distinguish between two broadly different types of igneous rock suite. These were the cordilleran, which "always formed during a period of great lateral pressure", and the laccomorphic, which was "not accompanied by marked lateral pressure". Benson made no attempt to relate these categories to the petrology of the rocks concerned, but nevertheless recognized that the two different tectonic settings might well result in different processes of magmatic differentiation. Until very recently, however, Benson's distinction between tectonic settings tended to receive less attention than the superficially more obvious contrast between the Atlantic and Pacific provinces. In other words, most attempts at relating petrology to tectonics have concentrated on the supposed differences between the chief oceans rather than on the possible differences between tectonic features within the oceans—or, to put it another

way, rather than on petrological similarities among similar tectonic features irrespective of location.

But in a paper which could well turn out to be one of the more important of recent years, Martin and Piwinski (*J. Geophys. Res.*, **77**, 4966; 1972) take, and develop, the point that Benson's definitions in terms of lateral pressures accord remarkably well with the concepts of the new global tectonics. Leaving aside transform faults, which have no associated magmatism and thus offer little scope for petrological correlation, modern lithospheric plates define two quite different tectonic settings—the convergent plate margin, where forces are predominantly compressional, and the divergent margin, which is largely tensional. These two settings, in turn, define orogenesis and the lack of it, for whereas regions of subduction are associated with orogenesis, tensional margins are unaccompanied by significant penetrative deformation and are thus essentially non-orogenic.

These ideas have led Martin and Piwinski to investigate the possibility that there may be significant petrochemical differences between orogenic-compressional-cordilleran zones and non-orogenic-tensional-laccormorphic zones. Such differences would be of interest in their own right; but, perhaps even more important, if they exist they could well help along the solution of other problems. For example, if correlations can be established for the relatively recent past, it may be possible to use them to distinguish tectonic settings relating to the period before the present drift episode—that is, beyond the past 200 million years or so. In view of the known difficulties in defining the plate patterns obtaining before the breakup of Laurasia and Gondwanaland, the potential of tectonic-petrochemical correlations is clear.

But do the necessary differences exist? Martin and Piwinski use data from four different igneous suites to show that not only do they exist, but at more than one level. The rocks in question were from the Aleutians (Alaska) and the Cascades (California, Oregon and Washington), representing emplacements along active oceanic-oceanic and oceanic-continental consuming plate margins, and from Ethiopia and Iceland, representing non-orogenic magmatism in both oceanic and continental environments.

Yet frequency plots for the four suites of the differentiation index (range 0 to 100) introduced by Thornton and Tuttle (*Amer. J. Sci.*, **258**, 664; 1960) clearly enabled the orogenic and non-orogenic rocks to be distinguished, irrespective of the geological environment. Thus both orogenic suites (Cascades, Aleutians) gave histograms single-peaked in the middle of the dif-

ferentiation index range (~35 to 65). By contrast, the non-orogenic Ethiopian and Icelandic suites produced bimodal distributions peaked at 20 to 30 and 80 to 90. As a result, Martin and Piwinski propound the view that bimodal distribution is typical of tensional regions irrespective of whether they are oceanic or continental. If this is really so, then it should be possible to use distributions of differentiation index to distinguish between orogenic and non-orogenic rock suites—and thus between compressional and tensional zones—even in pre-Wegenerian situations where other evidence of plate tectonic arrangements may not exist.

At a more subtle level, it may also be possible to make similar distinctions on the basis of detailed chemistry. If the percentage weight of a particular chemical constituent is plotted against the differentiation index, the trends in the variation diagrams are apparently similar for both orogenic and non-orogenic suites. But for some constituents the scatter of the data points is noticeably and significantly different. Thus for SiO_2 , Al_2O_3 , Na_2O and K_2O , in particular, the scatter for the non-orogenics is much greater than for the orogenics. It appears that CaO may also be a possible orogenic-non-orogenic discriminant in that the distributions within the AFM ternary diagram are quite different.

So far, the various methods of discrimination seem promising, although, as Martin and Piwinski point out, it would be wise to bear in mind that they may break down in specific instances. For one thing, a subduction zone may well be predominantly compressional but could easily have regions of local tension—for example, in the flexure zone where the lithosphere bends downwards. On the other hand, this sort of tensional region may rarely be magmatic (non-orogenic) because the open fractures may be limited to the upper part of the plate. But tension may also be characteristic of basins to the landward of oceanic trenches.

A second example is that of a lithospheric plate approaching a subduction zone, where the plate contains a spreading ridge. If such a ridge is still active it may well be non-orogenically magmatic even though it lies within the orogenically magmatic zone. Clearly in such a situation the two types of magmatism may be geographically and temporally confused. Yet another case is that of rock formed in a region of tension but subsequently remobilized in a region of compression. Ophiolite sequences, for example, may have been produced at ridge crests but then transported to, and remobilized in, subduction zones. Finally, there is, of course, always the problem of meta-

Metal Ions and Nucleic Acids

A NUMBER of metal ions have been shown to have a crucial effect on the biological activity of nucleic acids. It is hardly surprising therefore that there is an increasing interest in the stereochemistry of nucleic acid-metal ion interactions. Most of the stereochemical information obtained so far has come from spectroscopic and X-ray single crystal diffraction studies of complexes of metal ions with bases, nucleosides and nucleotides. The folklore from the X-ray work is that the alkali metal ions and the alkaline earths tend to be coordinated predominantly to phosphate oxygens, sugar hydroxyls and water molecules in the crystal, whereas Cu (II), Zn (II) and Hg (II) tend to interact preferentially with the bases. Unfortunately the X-ray studies so far include only a relatively small number of structure determinations, and while spectroscopic studies have been more extensive they tend to be somewhat more equivocal. Nevertheless the spectroscopic work does indicate that metal ion binding to nucleic acids and their monomers may be considerably more complex than the X-ray studies suggest.

A communication by Berger, Tarien and Eichorn in next Wednesday's

Nature New Biology (October 25) is of particular interest in this connexion. It describes proton magnetic resonance and optical spectroscopic studies of the interaction of the copper (II) acetate dimer with various ribonucleosides and deoxyribonucleosides. An X-ray single crystal study of the copper acetate dimer alone had shown that the two copper atoms are separated by 2.64 Å—a distance which matches rather precisely the separation of the 2'OH and 3'OH groups of a ribonucleoside sugar.

By studying nucleosides with a variety of sugars (including the arabinose form where the two hydroxyls are on opposite sides of the sugar ring) Berger *et al.* are able to show rather conclusively that in addition to interaction with the heterocyclic bases of the nucleosides, the two copper atoms in the copper acetate dimer interact in a stereochemically specific way with the two hydroxyls of a ribose sugar. As Berger *et al.* point out the significance of this study does not lie in the biological activity, if any, of copper acetate, but in more general considerations of the basis for stereochemical specificity in polynucleotide interactions and particularly in those biochemical mechanisms influenced by metal ions.

morphism which may modify the original petrochemistry in such a way as to obscure its orogenic or non-orogenic characteristics, especially in very old rocks.

But in spite of these complications, Martin and Piwinski maintain that their basic thesis is valid—that petrochemistry may be used to distinguish between compressional and tensional environments because the tectonic setting controls, to some extent, the nature of the corresponding magmatic differentiation.

RNA POLYMERASE

Enzyme Run to Earth

from our Cell Biology Correspondent

IN five consecutive papers occupying eighty-odd pages of the current issue of the *Journal of Molecular Biology* Chamberlin and his colleagues report an analysis of the binding of *Escherichia coli* DNA-dependent RNA polymerase to phage T7 DNA which matches in its thoroughness the analysis Kornberg's group have made of *E. coli* DNA polymerase I and which sets a standard others working with RNA polymerases would do well to emulate. The problem Chamberlin and his colleagues, and in particular a former graduate student Hinkle, have been intrigued by for the past several years is simply how does RNA polymerase select the sites on DNA templates at which to begin transcription? The work of several groups has revealed that in appropriate conditions the process of site selection for initiation of transcription can be investigated *in vitro* and that a dissociable subunit of RNA polymerase, sigma factor, has an essential role in the selection of promoter sites and the initiation of asymmetric transcription; but what is the mechanism that achieves these results?

Hinkle and Chamberlin (*ibid.*, 157) have modified a membrane filter assay which allows them to measure the binding of holoenzyme (with sigma) and core enzyme (lacking sigma) to phage T7 DNA. Using this assay they established that both core and holoenzyme can bind to T7 DNA. About eight molecules of holoenzyme bind to form a very stable complex and many more molecules bind to form comparatively weak complexes. By contrast core enzyme binds at many sites but yields comparatively unstable complexes. They believe therefore that core and holoenzyme have different conformations, the latter molecules being programmed for initiation, the former for elongation. Hinkle and Chamberlin (*ibid.*, 187) then investigated the kinetics of binding and, knowing the rate constants, propose the following model of

the selection of initiation sites by holoenzyme. They envisage that holoenzyme binds at many so-called B sites which are easily dissociable. The enzyme eventually encounters a tight binding or A site and can then form a stable complex with the DNA preparatory to initiation. The half time for the location of A sites is about 20s. In other words in the presence of sigma the enzyme first recognizes the T7 DNA and then gropes along the molecule by a series of associations and dissociations until it bumps into a tight binding site at the promoter region.

Tight binding *per se* is not, however, sufficient for chain initiation because as Hinkle, Ring and Chamberlin (*ibid.*, 197) have shown holoenzyme binds tightly to nicked T7 DNA but initiation occurs at very few single strand breaks. In other words tight binding is necessary but not sufficient for initiation of RNA synthesis. By contrast core enzyme transcribes nicked DNA more efficiently than native DNA so single strand breaks, irrespective of their location, act as initiation sites for core enzyme.

The antibiotic rifampicin, as several groups have shown, inhibits the initiation of transcription in bacteria. As Hinkle, Mangel and Chamberlin (*ibid.*, 209) have demonstrated, however, rifampicin neither prevents tight binding of holoenzyme nor increases the rate of dissociation of holoenzyme/DNA complexes. Furthermore the initiation of transcription by holoenzyme complexes is markedly less sensitive to inhibition by rifampicin than is transcription by core complexes. This difference presumably reflects the fact that holoenzyme initiates at a much faster rate than core enzyme and is comparatively protected against rifampicin. As Hinkle *et al.* point out, this means rifampicin can be used to discriminate rapid specific initiation from slow "false" initiation.

Finally Chamberlin and Ring (*ibid.*, 221) followed initiation by measuring incorporation of $\gamma^{32}\text{P}$ ATP and GTP in 2 min periods. About 70 per cent of holoenzyme molecules can initiate *in vitro* and the ratio of RNA chains initiated by ATP to those initiated by GTP does not vary with variations in ionic conditions. Apparently each T7 DNA has eight tight binding sites but many more holoenzyme molecules initiate per genome so these sites must be rapidly used again. Moreover in the presence of high concentrations of rifampicin two chains initiated by ATP and one by GTP are made. It seems likely therefore that the T7 early promoter regions contain at least three distinct initiation sites, which has interesting implications not least for those intent upon isolating and sequencing a promoter region.

SUPERCONDUCTORS

Fountain Effect ?

from a Correspondent

IT seems that low temperature physicists will not be content until the superfluid properties of helium (^4He) match perfectly the superconductive properties of pure metals. At a descriptive level, the frictionless flow of the superfluid component of ^4He is regarded as analogous to the zero-resistance electric current in a superconductor. As an example of a deeper similarity, the Josephson effect and its superfluid analogue have been discussed convincingly by Anderson. From a theoretical point of view, the similarities rest upon the common hydro/thermodynamical description of a superfluid defined as having a component which flows without macroscopic transfer of entropy. For the superconductor the fluid is charged so that electrical and magnetic effects must also be considered.

In a recent study, J. Clarke and S. M. Freake have observed certain asymmetries in a superconductive Pb-Pb junction and have ascribed these to a thermoelectric analogue of the thermomechanical effect in superfluid helium (*Phys. Rev. Lett.*, **29**, 588; 1972). This is sometimes known as the fountain effect (Allen and Jones, *Nature*, **141**, 243; 1938). Briefly, when helium flows out of a vessel through a capillary into a receiving vessel the temperature of the helium supply rises while the temperature in the receiving vessel falls. The effect is reversible (unlike that occurring in a normal fluid) so that if an external temperature difference is maintained, a pressure difference results, leading to a flow of normal fluid from warm to cool, compensated by a superfluid flow from cool to warm.

In the superconductive analogue, the two connected helium supplies are replaced by an electrical junction with a temperature difference between the contacts which causes a difference of chemical potential (corresponding to the pressure difference). The analogy now falters because of the essentially electrical nature of the superconductive fluid flow. Under steady conditions there can be no net electrical driving force for the normal component (on a microscopic level the quasi-particle current) so that the chemical potential gradient becomes the source of an equal and opposite electric field. The normal current is now produced solely by the net diffusion between the hot and cold contacts and is therefore proportional to the temperature difference, the coefficient being the product of the quasi-particle conductivity and thermopower. As with the thermomechanical effect, this normal current is compensated by an equal but opposite supercurrent, and

it is the latter which is actually measured.

The Clarke-Freake experiment was made with a Josephson junction consisting of a Pb wire making point contact with a Pb foil, the metal being extremely pure. The wire or the foil could be independently heated and maintained at selected temperatures below the critical temperature of Pb (about 7.19 K). The idea, briefly, is that if the junction critical current is sought by passing an external current across the junction, then a smaller or larger critical current will be needed, depending on whether the applied current is assisted or hindered by the intrinsic supercurrent produced by the temperature difference. Thus by reversing the direction of the applied current and/or reversing the hot and cool contacts, an asymmetry is expected.

To obtain significant differences, the experiment had to be finely adjusted since, assuming a junction resistance of 1Ω and a typical thermopower of $0.2 \mu\text{V K}^{-1}$, a 1 K temperature difference would produce current of only $0.2 \mu\text{A}$ compared with mean critical current of 1–15 μA . For this reason, measurements were made close to the critical temperature. In this region the critical current decreases very rapidly with temperature and thus permitted a range of values for an approximated constant temperature difference.

The difference between the critical current flowing from wire-to-foil, and that from foil-to-wire, with the temperature difference in either sense, agreed in sign with the theoretical prediction. The magnitudes varied from 8 times less to 7 times greater than the mean value as the junction contact was adjusted, but were largely independent of the mean critical current as expected. One unexplained and possibly significant result was that a comparable difference remained even for zero temperature difference.

Less exciting explanations of the observed asymmetry are possible. The critical current-temperature relationship need not be identical for wire-and-foil so that if perhaps the junction critical current is merely the lower of two alternatives, a similar asymmetry would be recorded. This situation could possibly be avoided by devising a symmetrical contact. At its best, the experiment confirms the thermoelectric behaviour assumed by theorists for some years.

BOTANY

On Plant Protoplasts

from a Correspondent

THE First International Symposium on Protoplasts and Fusion of Somatic Plant Cells was held in Versailles between September 11 and 15 under

the auspices of the Centre National de la Recherche Scientifique and the Institut National de la Recherche Agronomique. The symposium was particularly useful because it brought together both plant and animal workers concerned with the fusion of cells and allowed an exchange of viewpoints and ideas often missing in many strictly zoological or botanical discussions on the subject. In all, more than 100 researchers from many different disciplines attended.

The central theme which emerged from this symposium is that there is now a great opportunity for using protoplasts to increase the genetic variability of plants. And the need for more work in this connexion using economically important crops such as the cereals was also uppermost in the minds of the participants.

Problems associated with the isolation of protoplasts by the enzymatic degradation of the cell walls of plant tissues and cell and tissue cultures could be critically assessed against the background of reviews of the most recent work on the structure of the primary cell wall by Dr P. Albersheim (University of Colorado), including that of the protein-glycan network by Dr D. T. A. Lamport (Michigan State University). This enabled Dr K. Selby (Lord Rank Research Centre, High Wycombe) to guide aspiring isolators of protoplasts in their choice of the most

suitable cellulases and other enzymes for degrading cell walls. The culture of these naked cells was discussed by many participants and it was clear that there was considerable variation between species in the readiness with which they undergo both regeneration of the cell wall and division. Isolated leaf protoplasts normally regenerate a wall more readily than those isolated from callus or suspension cultures, and both Mr J. H. M. Willison (University of Nottingham) and Dr Rolland (Biologie Cellulaire, Paris) were able to describe the detailed changes in fine structure involved.

It was generally accepted that difficulties still remained in the isolation of protoplasts from certain tissues and in the culture of protoplasts, particularly those from cereals. Most workers were clearly convinced that the chief interest now centres on what can most usefully be done with these naked cells which cannot be done with the usual tissue cultures or suspension culture cells. As discussed by Dr I. Takebe (Institute for Plant Virus Research, Chiba, Japan) and by Dr O. L. Gamborg (NRC, Saskatoon) it is now established in several laboratories that it is possible to regenerate whole plants from single isolated protoplasts of both tobacco and carrot. Interest, therefore, naturally centred on the exploitation of this single cell system, particularly in relation to fusion and somatic

Induction of EBV Antigens

THE aetiological relationship between Epstein-Barr virus—a herpesvirus—and Burkitt's lymphoma and nasopharyngeal carcinoma remains a vexed question. There is, to be sure, ample evidence that infection by Epstein-Barr virus is a concomitant of these two malignancies, but proof that infection by this virus precedes and causes the development of the cancers is still lacking. One thing is certain, however, as DNA hybridization experiments have established, cells from patients with Burkitt's lymphoma and from healthy people may contain Epstein-Barr virus DNA even though it is impossible to detect any evidence of the multiplication of the virus in these cells. And as Sugawara, Mizuno and Osato report in *Nature New Biology* next Wednesday (October 25) such cells probably contain the entire Epstein-Barr virus genome.

Sugawara and his colleagues have worked with three human cell lines, the Raji line from a patient with Burkitt's lymphoma, the P3HR-1 line also from a Burkitt tumour, and the NC-37 from an apparently healthy donor. Only cells of the P3HR-1 produce Epstein-Barr virus and when these cells are grown in media containing IUdR or

BUdR the proportion of cells containing viral early antigens and capsid antigens increases many fold; in other words the proportion of cells producing virus increases markedly. Furthermore both Raji cells and NC-37 cells cultivated in the presence of these drugs become positive for both early antigens and capsid antigens of Epstein-Barr virus. And when fresh clones of these cells were exposed to the inducing drugs all became antigen positive.

IUdR and BUdR have, of course, recently been used to induce the expression of latent RNA tumour virus genomes in avian and murine cells. Sugawara *et al.* have now used these drugs to reveal latent Epstein-Barr virus genomes in human cells and, since after exposure to these drugs the human cells are found to contain both early viral antigens and viral capsid antigens, it seems highly likely that the entire Epstein-Barr virus genome is retained in a "repressed" state in Raji and NC-37 cells. Obviously such findings are consistent with the idea that Epstein-Barr virus may have a role in the aetiology of Burkitt's lymphoma, but it must be borne in mind that NC-37 cells were derived from a person without any trace of malignant disease.

hybridization, the uptake of nucleic acids and viruses, and any new evidence for transformation in higher plants. In this connexion, the viewpoints of Professor B. Ephrussi (CNRS, Gif-sur-Yvette) and Professor G. Pontecorvo (Imperial Cancer Research Fund, London) were useful, because provided that general methods are available for the selection of hybrids, the system provides a key approach to fundamental problems in animal and human cell biology even though at times the frequency of fusion of animal cells and subsequent hybrid cell formation may be very low.

Professor E. C. Cocking (University of Nottingham) emphasized that this same key concept of adequate selection applied to the use of protoplasts whether cell modification was being attempted by fusion or by uptake. Conditions for the fusion of tobacco leaf and petunia and crown gall callus protoplasts were comprehensively described by Dr J. B. Power (University of Nottingham); Dr P. K. Evans (University of Nottingham) surveyed the situation with cereal protoplasts and Miss Withers (University of Nottingham) presented the first electron microscope study of their induced fusion. The indication from the work of Dr K. L. Giles (DSIR, Palmerston, New Zealand), using maize chloroplast mutants, of complementation as a result of fusion attracted great interest. With these fusion procedures, Dr P. S. Carlson (Brookhaven National Laboratory) showed, by means of a special nutritional selection procedure, how it is possible to select out somatic hybrid cells at a frequency of only a few in a million and to regenerate from them whole somatic hybrid plants.

From the studies of Dr Ohyama (NRC, Saskatoon) it seems likely that protoplasts do possess real advantages in uptake studies using DNA, for the absence of the cell wall results in less degradation of the input DNA. While it was clear that no transformation has as yet been obtained using isolated protoplasts, the uptake of whole bacteria into protoplasts described by Dr M. R. Davey and Professor E. C. Cocking (University of Nottingham) attracted great interest in view of the fact that the behaviour of bacteria in single protoplasts capable of undergoing division to form calluses and, ultimately, plants can now be studied.

MOLECULAR BIOLOGY

Fraction I Protein

from a Correspondent

A SINGLE functional protein of the chloroplast, Fraction I, apparently consists of subunits some of which are coded in the nuclear DNA and probably synthesized in the cytoplasm, while the

others are coded in the chloroplast DNA and synthesized within the chloroplast.

The genetic and biochemical autonomy of organelles has become a highly fashionable subject in recent years, and a great deal of research has been directed towards an understanding of the interrelationships between the nucleus, the cytoplasm, and the organelles. Perhaps the most intriguing questions here are whether organelle proteins are coded in organelle or nuclear DNA, and whether they are synthesized on cytoplasmic or organelle ribosomes.

Clearly, the most promising approach to these questions is to study the synthesis and mode of inheritance of a single, identifiable organelle protein like Fraction I, which can comprise up to 50 per cent of total leaf protein in certain plants, carries ribulose-1,5-diphosphate carboxylase activity, has a molecular weight of approximately 525,000 and consists of large and small subunits.

In a recent report Pak-Hoo Chan and Wildman (*Biochim. Biophys. Acta*, **277**, 677; 1972) have produced strong evidence that the large subunit is coded for by the chloroplast DNA. Their work is based on a difference of a single polypeptide in the tryptic digests of large subunits from Western Hemisphere and Australian species of *Nicotiana*. By reciprocal crosses, they have shown that the specific polypeptide of the Australian species can only be transferred to hybrids from the maternal parent. As maternal inheritance is taken to imply the involvement of extranuclear genes they have reasonably concluded that the genetic information for the large subunit is coded in the chloroplast DNA. A similar approach was used earlier this year by the same group to determine the site of coding of the small subunit (Kawashima and Wildman, *Biochim. Biophys. Acta*, **262**, 42; 1972). Differences were detected in the tryptic digests of the small subunits of Fraction I in three Western Hemisphere species of *Nicotiana*, but the information for these differences could be transmitted by way of the pollen, indicating that the nuclear DNA codes for the small subunit.

Information has also come to hand this year concerning the site of synthesis of at least the large subunit. Earlier reports on the differential labelling of the two subunits *in vivo* (Kawashima, *Biochem. Biophys. Res. Commun.*, **38**, 119; 1970) and of the sensitivity of the labelling of the large subunit to chloramphenicol, and of the small subunit to cycloheximide (Criddle, Dau, Kleinkopf and Huffaker, *Biochem. Biophys. Res. Commun.*, **41**, 62; 1970) suggested that the large subunit was synthesized on the chloroplast ribosomes, and the small subunit on cyto-

plasmic ribosomes. This view was shown to be correct earlier this year when Blair and Ellis (*Biochem. J.*, **127**, 42P; 1972) reported, in abstract only, that isolated illuminated chloroplasts incorporated ^{14}C -leucine into a single labelled product which migrated on SDS-acrylamide gels exactly with the large subunit of Fraction I.

This fascinating protein undoubtedly still has a lot to reveal, especially about the mechanisms regulating the synthesis of the two subunits, and about the endosymbiosis concept of organelle evolution.

SOLID STATE

Defects Discussed

from a Correspondent

ABOUT 150 physicists and metallurgists, more than half of them from overseas, gathered at the University of Sussex from September 18 to 20 to discuss point defects and their aggregates in metals. An erudite gathering, one might think, far removed from the cares of the real world; but behind this academic facade lies the knowledge that a good understanding of processes involving point defects is the key to many materials problems which arise in practical situations. In particular, the behaviour of materials in nuclear reactors is dominated by point defects and their aggregates produced by bombardments with nuclear particles, and the economics of nuclear power is intimately bound up with the lifetime of these materials. It is no accident that more than a third of the delegates are employed in establishments such as AERE at Harwell, Centre Etudes Nucléaires (CEN) at Grenoble, or the Kernforschungsanlage (KFA) at Jülich.

The international character of this meeting, run by the Institute of Physics, was undoubtedly a consequence of the fact that it is four years since the last large meeting in Europe on this topic, held at the KFA at Jülich in 1968. In many ways four years is a long enough period to measure the progress and to note the changing concerns of the field as well as to identify the on-going problems. The highlights of the present meeting were new techniques which were not well developed in 1968. In addition, it was clear that work using standard techniques, such as electrical resistivity, had increased enormously in scope and in concern for minute detail as compared with four years ago.

The principal new techniques which have been developed during this period are positron annihilation, field ion microscopy (FIM), high voltage electron microscopy (HVEM) and X-ray diffuse scattering. Six groups presented work in which positron annihilation was used to study vacancies, vacancy-impurity

complexes, voids and dislocations. Positrons can be trapped at all these defects and decay with a longer lifetime than in the perfect crystal, because of the lower electron density in the neighbourhood of the defect. The angular correlations or the line width of the decay γ rays can also be measured. By measuring the lifetime (or other properties) as a function of temperature, vacancy concentrations can be deduced. These techniques complement older equilibrium techniques, and vacancy parameters deduced by several groups were in substantial agreement with each other and with older observations. A very considerable increase in positron lifetime was found by one group for samples containing voids, and another group is attempting to deduce binding energies of vacancy-impurity complexes in alloys. Although the chief effects in this new field are now clear, many details certainly remain to be unravelled, and the accuracies of parameters deduced seem rather uncertain at present.

A most impressive account of FIM work on point defects in irradiated and quenched metals was given by Professor D. N. Seidman (Cornell University). The FIM has emerged as a quantitative technique in recent years; an automated data collection and analysis system, such as the one developed at Cornell, is essential because of the extremely low proportion ($\lesssim 10^{-3}$) of defective lattice sites present, which must be individually counted in this technique. A ciné film was shown of the damaged regions after heavy ion bombardment and of the annealing of single interstitial atoms in tungsten.

The development of HVEM has clearly contributed much to the study of clusters of point defects, presently spurred on by the observation of voids (three-dimensional vacancy clusters) in materials used in fast reactors. Irradiation in the HVEM can be used to accelerate these processes and to study the defects produced under a wide variety of conditions. Other clusters can also be studied at very high resolutions using the recently developed weak beam technique. Analytical and quantitative interpretation of the pictures obtained has become much more important in the past few years.

The developments of the theory and practice of diffuse X-ray scattering complement the HVEM observations, for this scattering is sensitive to cluster sizes that are too small to be seen in the electron microscope. In a review by Professor W. Schilling (Jülich), he showed that this technique reveals the coarsening of interstitial clusters which occurs when irradiated metals are warmed up from low temperatures; the symmetry of the strain field of the defects can also be determined.

A conference such as this would not

be complete without the continuing saga of whether two types of interstitial atoms exist in simple metals or only one. This minor yet persistent topic has all the characteristics of a paradigm debate in the Kuhnian sense, with the two sides talking through each other, selecting different data as important and having different criteria for satisfactory explanations. This state of affairs has persisted as long as it has because the field is complex and because of spirited, yet incomplete, defences on both sides. The new theoretical and experimental techniques discussed at this conference may well be able to resolve these problems of interpretation, at the same time as providing the more accurate defect parameters which are needed in many practical materials applications.

NUCLEAR QUADRUPOLE RESONANCE

A Step Forward

from a Correspondent

THERE has been a considerable revival of interest in nuclear quadrupole resonance in the past two or three years, if the number of people attending the recent International Symposium on Nuclear Quadrupole Resonance Spectroscopy at Queen Elizabeth College, London, between September 28 and 29 (about 120) is anything to go by.

Although the first nuclear quadrupole

resonance experiment was performed more than twenty years ago by Dehmelt and Krieger in 1951, the technique has never acquired the popularity of later techniques such as nuclear magnetic resonance, Mossbauer spectroscopy and electron spin resonance. This is a pity because any isotope of an element with a spin of 1 or greater has a nuclear electrical quadrupole moment, and in principle quadrupole resonance spectra can be observed in its compounds. This means that more than 75 per cent of the elements in the periodic table are candidates for study. The resonance frequencies of the signals too are remarkably sensitive to the chemical and crystal environments of the atoms. Thus for chlorine the ^{35}Cl signals range from near zero in ionic chlorides such as NaCl to 54.1 MHz in molecular chlorine, with most carbon-chlorine bonds giving signals between 30 and 40 MHz.

The great difficulty has been the detectability of these signals; the signal-to-noise ratios have often been very low and the problem has been to spot the signal amongst the background "grass".

To judge from the papers presented at the symposium, however, the situation is now much happier. Professor J. A. S. Smith (Queen Elizabeth College), who organized the conference, presented an extremely interesting paper

Abnormal Haemoglobin Synthesis

IN *Nature New Biology* next week (October 25) Rowley and Kosciolk report a set of experiments which strongly support the idea that the hereditary anaemia, β -thalassaemia, which is characterized by a slow or negligible synthesis of β -globin chains, may result from more than one sort of genetic defect in the machinery for synthesizing proteins in immature red blood cells. Persons from the Ferrara region of Italy who are homozygous for β -thalassaemia fail to make β -globin chains. When, however, cell-free systems obtained from the reticulocytes of Ferrara β -thalassaemics are supplemented with supernatant factors from the cells of normal people β -globin chains are made. Presumably therefore the Ferrara β -thalassaemia is not the result of a defect in β -globin mRNA molecules but is some essential translation factor.

By contrast Rowley and Kosciolk have found that cell-free systems from the reticulocytes of Sicilian and Neapolitan β -thalassaemics do not respond to added supernatant factors; this suggests that β -thalassaemia in these southern Italians results from a defect in β -globin mRNA molecules rather than a defect in a translational factor.

As they point out, similar investigations of Negro β -thalassaemics will be of particular interest now that it seems that the syndrome in different populations may stem from different sorts of genetic defects.

The precise nature of the translational factor which is defective in Ferrara β -thalassaemics remains to be elucidated, and the experimental system which Ostertag and his colleagues describe in the same issue of *Nature New Biology* may well prove useful to those determining the minutiae of globin synthesis. They have found that when leukaemic spleen cells infected with Friend leukaemia virus are cultivated in the presence of low concentrations of DMSO the cells begin to differentiate along the erythrocyte line and reach a stage at which as much as about 25 per cent of the newly synthesized soluble cytoplasmic protein is α and β -globin. Furthermore the appearance of globin correlates with the appearance of a new 9S species of cytoplasmic RNA which is no doubt globin messenger. It would be interesting to test whether or not supernatant fractions from these murine cells would stimulate β -globin synthesis in cell-free systems from Ferrara β -thalassaemics.

illustrating the use of both frequency sweep and pulse techniques to study compounds containing the hydrogen dichloride, HCl_3^- , ion and its deuterated analogue. The frequency of the signal in a number of compounds was recorded and the relaxation time of the effect measured.

Another paper, which represented a very considerable breakthrough, was given by Dr D. T. Edmonds (University of Oxford) who reported the application of a low-temperature consecutive double-resonance experiment to detect ^{14}N and ^{2}D pure quadrupole resonance. The work on nitrogen was particularly interesting, for this technique seems to give a sensitivity at least two orders of magnitude greater than that obtained by more conventional techniques. This means that the ^{14}N n.q.r. of quite complex molecules such as amino-acids and nucleotides can now be examined. Other papers by Dr M. Read (University of Liège, Belgium) and Dr T. Oja (University of Denver) reported the use of computer averaging and pulse techniques to obtain better spectra. A paper by Professor C. T. O'Kouski (University of California, Berkeley) discussed the prediction of the effect of hydrogen bonding on ^{14}N nuclear quadrupole coupling constants by means of *ab initio* calculations, and the checking of the accuracy of such calculations by experimental n.q.r. measurements. Clearly at least for many systems containing nitrogen, ^{14}N n.q.r. provides a good check for the behaviour of calculated wave functions near the nucleus.

Dr H. G. Fitsky (Bayer AG, Leverkusen, West Germany) discussed the use of ^{35}Cl n.q.r. resonance frequencies as a structural tool in the study of organochlorine compounds. Structure determinations in these compounds, short of full X-ray single crystal studies, have proved notoriously difficult. The possibility of using $\text{C-}^{35}\text{Cl}$ n.q.r. frequencies to define "chemical shifts" in the same way that proton magnetic resonance has been used to define chemical shifts, and of using these shifts empirically in structural studies, is a very attractive one.

TOXICOLOGY

International Workshop

from a Correspondent

THE Second International Workshop on Toxicology was held at the University of Surrey from September 18 to 22. It was an extremely useful and practical workshop, and there can be little doubt that the ivory towers of academia, at Surrey at least, are well

below the clouds. This workshop establishes the Department of Biochemistry, which is headed by Professor D. V. Parke, as a *force majeure* in the field of toxicology in Britain.

The emphasis of the entire conference was on the practical applications of the assessment of risk to biological systems. There is scant evidence that Western man is ingesting a potent hepatocarcinogen, yet much research money is being poured into investigations of this subject. But relatively little money is being spent to further the understanding of the toxicology of drug addiction, and none, other than from the industry itself, is being spent on research into a safer cigarette, this being a field of research in which Britain is far further advanced than any other country.

The difficulties of keeping a perspective in toxicology were discussed, particularly when political pressures are brought to bear on scientists to make decisions without the necessary facts, and without giving them the necessary time to acquire them. This is exemplified by the banning of cyclamates in the United States with such alacrity following the announcement by the Food and Drug Administration.

As expected from a department of biochemistry, there were several useful papers on drug metabolism and chemical carcinogenesis, and on the role that chemistry plays in the better understanding of drug interaction, animal species differences and bio-availability. This latter subject was of great interest in view of recent comments in the medical and pharmaceutical literature concerning the bio-availability of digoxin. The value of short-term against long-term tests was also fully discussed and a plea was made that the former should be pursued as far as possible because they enabled some potentially hazardous compounds to be screened within a prescribed time and financial framework.

It was felt that many valuable basic data on many species and much toxicological material now available within industry should be made publicly available so that background knowledge would be increased to the benefit of all parties.

The symposium was closed by Dr F. A. Fairweather of the Department of Health and Social Security, who is concerned with environmental pollution in its widest sense and who pointed out in a brilliant lecture some of the pitfalls that face a toxicologist, whether he is in an official agency or a commercial organization. He ended with a plea for closer collaboration between the official agencies and industry, a plea which shall not be left unheeded. Well done, Surrey, we look forward to the next meeting.

ANIMAL BEHAVIOUR

Spider Movement

from our Animal Behaviour Correspondent

If a jumping spider sees a small dark object with one of its four lateral eyes, it will turn its body so that it can scan the object with the two principal eyes at the front of its head. It is known (M. F. Land, *J. Exp. Biol.*, **54**, 119; 1971) that once the stimulus has been detected, the spider will turn by just the right amount even when it has no visual feedback from its movements, that is, the turning system is an "open loop" one. Therefore the size of the turn to be executed has to be specified in advance and the spider stops turning independently of whether the front eyes can actually see the object. This raises the very interesting question of how the size of the turn which the spider is going to make is specified and what are the instructions that are sent to the legs about how to turn and when to stop? In a recent paper, Land discusses some of the possible ways of doing this (*J. Exp. Biol.*, **57**, 15; 1972).

The possibility that the command is simply to turn for a specified length of time, depending on the position of the object, can be discounted by the observation that speed of turning is variable, so that instructions based on duration of a turn would not achieve the observed accuracy. A second possibility, that each pattern of target stimulation on the retina elicits a unique pattern of leg movement, appropriate for bringing the target to the front of the animal, can also be rejected, because the spider can begin a turn with its legs in any position and still turn accurately. A third possibility is that the spider computes, from information derived from the eyes about the position of the stimulus and proprioceptive information from the legs about their initial positions, what the final positions of the legs at the end of the turn should be, and turns until this position has been achieved. This, too, is unlikely as such a mechanism would be ambiguous—the same final leg positions would be specified if the turn were to be 75° or 150° , as one step by all the eight legs takes the spider through an angle of about 75° . Spiders show no evidence of confusing turns of different angles.

The most plausible explanation is that instructions to the legs concern the number of steps (leg movements) which have to be made. The angle through which the animal turns at each step is constant, over a ten-fold range of stepping speeds and an even greater range of inertial loading of the legs. Turning instructions that are specified in terms of a particular number of leg movements would therefore result in accurate turning.

The 5-km Radio Telescope at Cambridge

MARTIN RYLE

Mullard Radio Astronomy Observatory, Cavendish Laboratory, Cambridge

Radio astronomers can now map the sky with a resolution comparable to that of the best optical telescopes. This latest advance in aperture synthesis technology should yield important new evidence on the physics of radio galaxies, quasars and supernova remnants.

At a small ceremony on October 17, 1972, Sir Alan Hodgkin, President of the Royal Society, inaugurated the new 5-km telescope at the Mullard Radio Astronomy Observatory of the Cavendish Laboratory. This instrument, which was built with a grant of £2.1 million from the Science Research Council, is the latest in a series employing the principle of aperture synthesis which have been constructed at Cambridge since 1957. It is designed to provide radio maps with a resolution of 1 arc s which is comparable with that of optical telescopes.

The chief purpose of the telescope is to increase our understanding of the physical mechanisms occurring in radio sources, both galactic and extragalactic. For many of these sources, previous studies have had angular resolution sufficient to reveal the principal features of their structures but it has become clear that important details have remained unresolved, details which may well be vital when constructing successful theoretical models of the sources. With greater angular resolution one may hope, for example, to distinguish between the various proposed explanations of the double structure of the typical radio galaxy or quasar, to understand more clearly the interaction between an expanding supernova shell and the interstellar medium, and to gain insight into the problems of star formation from a study of the compact radio "knots" which occur in dense clouds of ionized hydrogen and are closely correlated with infrared and molecular line sources. The telescope will also be a powerful tool for radio astrometry since it is expected to provide not only extremely accurate positions of compact radio sources associated with faint optical objects but also a primary reference system of celestial positions, comparable in accuracy with the systems already established by traditional optical techniques.

High Resolution Mapping

There are several approaches to the problem of high resolution mapping in radio astronomy. First, the use of precision dishes operated at the shortest radio wavelengths; in this the mechanical properties of materials set a lower limit of about 1 arc min to the beamwidth attainable. Second, the method of lunar occultations which has proved very valuable in the case of compact sources, but which can only provide unambiguous maps of more extended sources if large numbers of occultations are observed. Occultations only occur, of course, for sources lying near the ecliptic. Third, aperture synthesis¹ in which two or more small aerials are moved to occupy in turn the

positions of the elements of a much larger aerial. By combining with a computer the signals recorded in these different positions it is possible to obtain maps having the angular resolution of the larger aerial.

To avoid the complications of physically moving the dishes in two coordinates to cover the required plane, use may be made of the variation of the angle of projection of a fixed axis as the Earth rotates. A particularly simple design uses two or more aerials mounted on an east-west baseline; if observations of a given area of sky are continued for a period of 12 h, the rotation of the Earth results in the synthesis, for each pair of aerials, of a ring of a large aperture parallel to the equatorial plane. By repeating the observations with different spacings of the small aerials the full aperture may be synthesized².

Present Instruments

The first major instrument of this kind was the One-Mile telescope, completed at Cambridge in 1964³, which was initially operated at wavelengths of 73 and 21 cm to provide maps of individual sources and to extend the study of very faint sources to flux densities some 100 times smaller than had been detected hitherto. Subsequently it has been used at wavelengths of 11 cm and 6 cm to produce more detailed maps (of 12 and 6.5 arc s resolution) of individual sources^{4,5}.

Other instruments of similar design have been built subsequently. These include the Half-Mile telescope at Cambridge, intended primarily for mapping the distribution of neutral hydrogen in nearby galaxies, and the Westerbork telescope of the Netherlands Foundation for Radio Astronomy, which has a resolving power similar to that of the One-Mile telescope but higher sensitivity. Several others are under construction: at the University of Sydney, New South Wales; at Stanford University, California, and at the Dominion Radio Astrophysical Observatory, British Columbia. In addition, the principle of aperture synthesis has been applied to telescopes not specifically designed for this purpose, notably those at the Owens Valley Radio Observatory of the California Institute of Technology, and the US National Radio Astronomy Observatory (NRAO) at Greenbank, W. Virginia. It is proposed that the VLA (Very Large Antenna) project of NRAO will also use this principle.

Design of the New Telescope

There are limitations to the resolving power of any radio telescope on the surface of the Earth set by the fluctuations of phase across the incident wavefront introduced by atmospheric irregularities; at lower frequencies these fluctuations are caused chiefly by ionospheric irregularities, whilst at higher frequencies they are caused by turbulence in the troposphere. Although considerable information is available on ionospheric irregularities, little was known until recently about tropospheric phase fluctuations. An extensive programme of observations was therefore carried out with the One-Mile telescope to investigate their magnitude and lateral scale size

as a function of time of day and year and under various weather conditions. The results⁶ indicate that, for 50% of the time, angular resolutions better than 1 arc s should be attainable with instruments of 5–10 km aperture operating in the wavelength range 3–6 cm.

In the design, emphasis has been placed on achieving the maximum possible resolution for a given cost; because the extension of observations to sources still fainter than those already investigated for cosmological purposes was not envisaged, relatively small but more precise dishes were selected to allow operation at shorter wavelengths. It is essential,

largest maps which may be made is less than that of the field of view of the individual dishes, although at a wavelength of 6 cm it exceeds 5 arc min; this restriction was accepted in view of the high cost of the deep piled foundations needed to obtain the required positional stability. A stability of about 1 mm is required for the dish foundations and structures, and for the electrical lengths of the coaxial cables linking the dishes to the correlation receivers in the control building.

The dishes are equatorially mounted with their feed-horns at the Cassegrain focus. The reflector surfaces depart from the usual paraboloid-hyperboloid system to provide a better



Fig. 1 Layout of the 5-km telescope. The length of the rail track is about 1.2 km and equals the spacing between the fixed aerials.

nevertheless, to maintain a reasonable sensitivity as the resolution is increased, because the total flux density from a source is spread over correspondingly more independent areas of the map.

The original design was based on a total aperture of 5.1 km, but the subsequent proposal to build the M11 motorway on a line crossing the site (which had already been agreed to by the Ministry of Housing and Local Government) necessitated a change of design and a reduction of aperture. The final arrangement (Figs. 1 and 2) employs eight 13-m dishes

distribution of aperture illumination; in this way the forward gain is improved and the ground radiation reduced. They were built by the Marconi Company and are based on a design for a satellite communication station. The surface accuracy and secondary reflector design are such as to allow efficient operation over the range of wavelength 3–21 cm, which corresponds roughly with the useful range for a 5 km aperture as limited by the atmospheric irregularities and provides angular resolutions of 1 to 7.5 arc s. Although some of the most important observations are likely to be those with

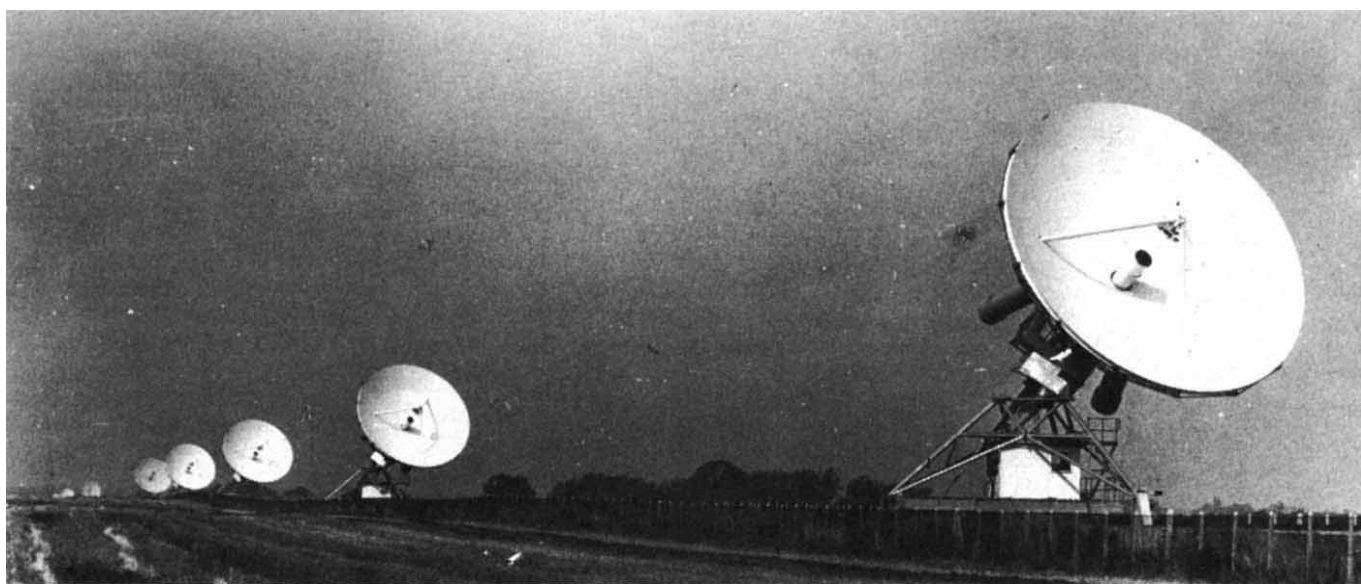


Fig. 2 The four moving aerials are in the foreground; the most distant fixed aerial can just be seen.

arranged on an east-west axis 4.6 km in length so that, by combining the signals from each of the four movable aerials with those from the fixed ones, sixteen spacings are available simultaneously; by transferring the four movable aerials to other positions along the rail track, up to 128 different spacings can be used for any particular observation. The number of spacings determines the maximum angular extent of the sources which can be mapped without disturbance from the first grating response. Unlike the One-Mile, Half-Mile and Westerbork instruments, the increment of spacing is larger than the diameter of the dishes, so that the angular diameter of the

the maximum resolving power, maps at lower frequencies (and lower resolution) will be important both in a search for differences in the distribution of total brightness and polarization with frequency and also in the study of features of low surface brightness where the sensitivity with the full resolution is insufficient.

The receiving system used for the initial observations operates at a wavelength of 6 cm, giving an angular resolution of 2 arc s in right ascension and $2 \cos \delta$ arc s in declination. The signals, after amplification in a low-noise parametric amplifier (Ferranti), are mixed with a local oscillator signal derived from a sub-

harmonic transmitted to each aerial along air-spaced coaxial cable. This part of the receiving system is installed in an enclosure behind the dish and close to the feed-horn. The 45 ± 4 MHz IF signal from each aerial is transmitted back to the control building (Fig. 3) along similar coaxial cables; the cables for local oscillator, IF and those used to equalize the path from each pair of aerials are all buried at a depth of 1 m to reduce the short-term temperature variations.

The design and construction of the receiving system, including that part installed at the focus of each aerial, the correlation receivers and the path compensation system, were carried out by the staff of the Observatory, in particular by Dr P. F. Scott and Messrs D. W. J. Bly and B. Elsmore.



Fig. 3 Control room, showing telescope console and teletypewriter; the correlation receivers are in the racks in the background.

The telescope drives, the diode switches of the path compensation system, the rotation of the feed-horn for polarization measurement and the sampling of the outputs from the sixteen correlation receivers are controlled by an on-line computer ('Marconi Myriad II'). The computer also carries out the Fourier inversion and any further data processing on a time-sharing basis. The results may be displayed on a 30-inch graph plotter. Control of the telescope is by means of typewriter commands to the computer and a programme of several days' observations can be held in the store for subsequent execution. The Marconi Company provided the programmes for aerial steering, which form part of the extensive software needed for control of the telescope. Most of the software, including the computation of precession, the continual updating of delay path and phase, the Fourier inversion and mapping programmes, was written by Drs S. Kenderdine and G. G. Pooley and Mr D. M. Odell of the Observatory staff.

The instrument has been built along a section of the old Cambridge-Bedford railway, which runs almost exactly east-west and is adjacent to the existing Observatory. The subsoil consists of some 50 m of gault overlying about 20 m of greensand. The gault provides an excellent foundation for deep piling, but some long-term vertical movement may occur owing to variation of water content in the greensand; as indicated later, there is no difficulty in monitoring any such movement.

Calibration

The angular coordinates of most earlier radio telescopes have been established by the observation of small diameter sources identified with compact optical objects; the most accurate positions of these objects, most of which are faint, have been determined by using large optical telescopes to relate them to nearby stars whose coordinates relative to the fundamental stars must then be established using smaller optical

telescopes⁷. In the construction of the present instrument it was decided instead to establish the axis by a precise survey, since it was believed that a first-order precision theodolite might locate the axis directly in relation to the fundamental stars with comparable or better accuracy. This was carried out by the Ordnance Survey, with the assistance of the Mekometer group of the National Physical Laboratory for distance measurements. It included: (a) geodetic levelling, involving measurements to determine the deviations of the vertical;

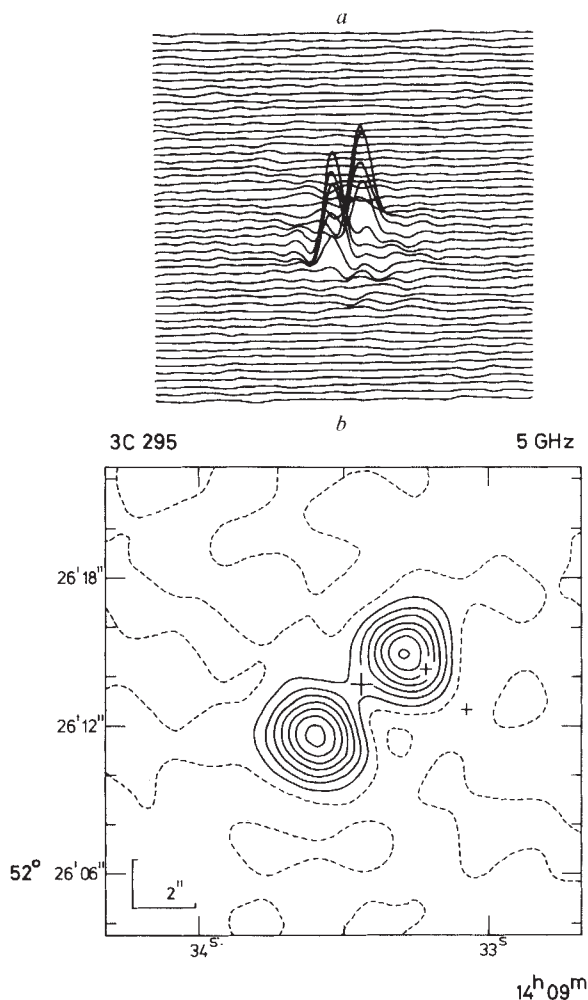


Fig. 4 Map of the radio galaxy 3C 295. *a*, Plotted as cuts across the contour map; *b*, plotted as a contour map; the arms of the L-shaped symbol are 2 arc s.

(*b*) alignment, using a new technique which necessitated working at the top of 10 m steel towers; (*c*) a series of astronomical observations to determine the latitude, longitude and precise azimuth of the baseline of the instrument, and (*d*) the measurement of the separation of the aerials along the baseline. The results of this very careful survey⁸ were used to locate the point of intersection of polar and declination axes for each dish in all three coordinates, with an accuracy of better than 10 mm relative to a line whose astronomical coordinates are known to within 0.2 arc s.

Radio observations of point sources (whose absolute positions did not have to be known) were then used to refine the position of each dish relative to the mean Ordnance Survey line; the maximum correction needed was ≈ 8 mm. This operation in effect "polished" the instrument to a figure of ± 1 mm. The telescope at this stage produced good images and could be used to determine positions with respect to the Ord-

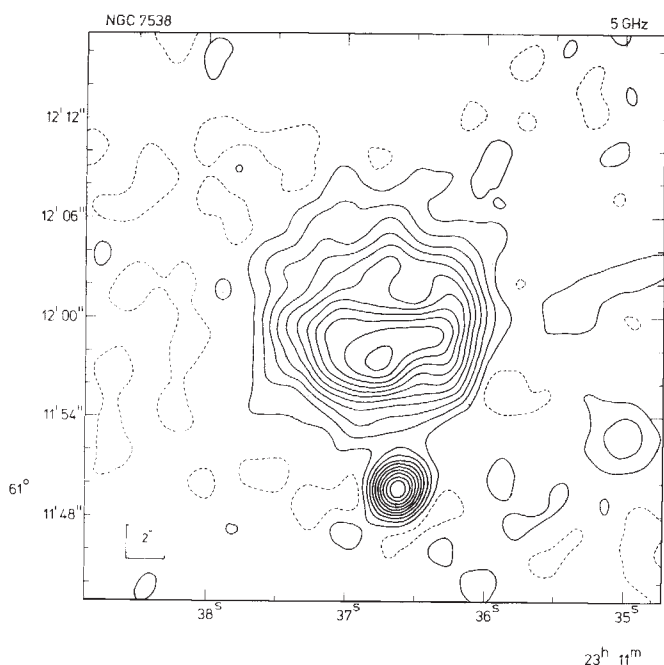


Fig. 5 Compact radio source associated with the H II region NGC 7538.

programme for determining the corrections for a number of the positions of the movable aerials is still not yet complete. Several maps of relatively compact sources have, however, been made and some of these are shown in Figs. 4-6.

Each of the maps shown was obtained in a single 12 h observing period and was available from the computer within a few minutes of completion of the observations.

Fig. 4, which presents two types of map, shows the radio galaxy 3C 295 which is already known from long baseline observations¹¹ to consist of two compact components 4.5 arc s apart and is a useful test source. The large cross indicates the position¹² of the cD galaxy associated with this source. From Fig. 1 of Minkowski¹³ its envelope appears to extend beyond the radio components. The positions of the two nearest galaxies in the cluster are marked by small crosses. Fig. 5 shows the compact source associated with the H II region NGC 7538 which will be discussed in detail elsewhere. The source 3C 86 has been known for some time to consist of two, probably unrelated, components 4.5 arc min apart. Fig. 6 is a map of the Nf field, which shows that 3C 86A consists of two fairly complex components. There is heavy obscuration in this region and no galaxies are visible optically.

I acknowledge the contributions of the Marconi Company Ltd, Mitchell Construction Company Ltd, British Insulated Callendar Cables and in particular the UK Atomic Energy Authority who were responsible for the design of the civil engineering work and building, and acted as agents for the Science Research Council for the project. In addition to those

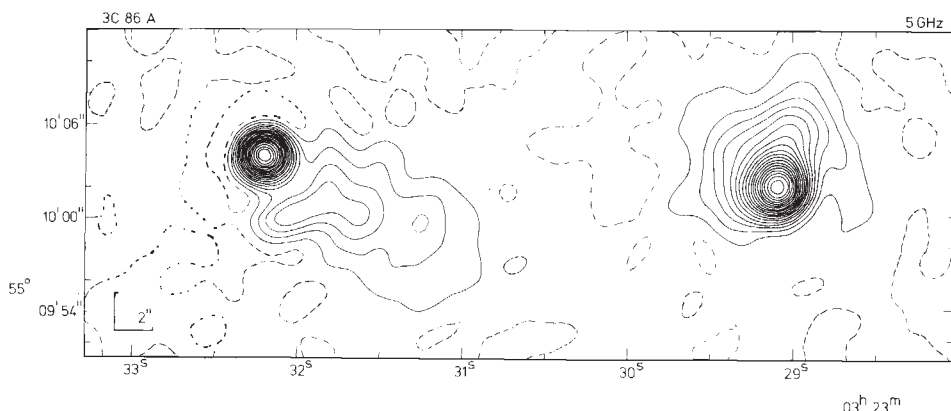


Fig. 6 Map of 3C 86A. The L-shaped symbol again indicates an angle of 2 arc s.

nance Survey line, to an accuracy of ~ 0.2 arc s in right ascension and 0.3 arc s in declination at $\delta = 45^\circ$.

During the construction of the instrument it was announced by Wade and Hjellming⁹ that radio emission could occasionally be detected from Antares; subsequent observations showed that β Persei (Algol) and other bright stars could also be detected¹⁰. Radio observations of these fundamental stars offer an independent method of establishing the absolute coordinates of the instrument, and a series of observations of β Persei (probably the most intense at radio wavelengths) has therefore been made. Although the flux density has only been sufficient for accurate measurements on five occasions since the completion of the telescope, these preliminary results have already enabled a calibration of the system relative to this star to better than 0.1 arc s in both coordinates. The results are in extremely good agreement with the Ordnance Survey values and correspond to differences in the derived coordinate systems of 0.1 arc s in right ascension and 0.15 arc s in declination at $\delta = 45^\circ$.

First Maps

Most of the time since the constructional work was completed has been spent in calibration as described above and the

members of the Observatory staff already mentioned, several others, particularly Mr R. Levente, have played an important part in the construction and testing of the instrument.

¹ Ryle, M., and Hewish, A., *Mon. Not. Roy. Astron. Soc.*, **120**, 220 (1960).

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⁶ Hinder, R., and Ryle, M., *Mon. Not. Roy. Astron. Soc.*, **154**, 229 (1971).

⁷ Murray, C. A., Tucker, R. H., and Clements, E. D., *Nature*, **221**, 1229 (1969).

⁸ Preiss, W. J., *Commonwealth Survey Officers Conference*, Paper No. B6 (1971).

⁹ Wade, C. M., and Hjellming, R. M., *Astrophys. J.*, **163**, L 105 (1971).

¹⁰ Hjellming, R. M., Wade, C. M., and Webster, E., *Nature*, **236**, 43 (1972).

¹¹ Anderson, B., Palmer, H. P., and Rowson, B., *Nature*, **195**, 165 (1962).

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¹³ Minkowski, R., *Astrophys. J.*, **132**, 908 (1960).

Large Outburst of Cygnus X-3

P. C. GREGORY

Scarborough College, University of Toronto

Professor P. C. Gregory, of the University of Toronto, describes the recent radio outburst of Cygnus X-3, and presents the overall picture of the source which has been gleaned from observations at many wavelengths. These observations are reported in full in the two articles following, and in a special issue of *Nature Physical Science* next Monday.

ON the evening of Saturday, September 2, my colleagues and I recorded at a frequency of 10.5 GHz the most impressive outburst ever witnessed by radio astronomers. The outburst was from the X-ray source Cygnus X-3 (*Nature*¹, this issue, page 440). The radio emission had increased 1,000 times above the level observed only two days earlier by radio astronomers at the US National Radio Astronomy Observatory (NRAO) (*Nature*², this issue, page 443).

News of the discovery was quickly relayed to colleagues at NRAO who confirmed the Canadian finding with measurements at 2.7 and 8 GHz. The two observatories then alerted others around the world. By early Sunday morning preliminary results had been received from four other observatories which enabled the radio spectrum to be plotted from observations at six frequencies between 1.4 GHz and 16 GHz. The result looked like a beautiful example of the spectrum from a thermal source having a temperature of $\sim 10^8$ K. This fact led us to predict that the outburst might have been observed at X-ray wavelengths as well. By good fortune the spin axis of the Uhuru X-ray satellite was pointing in the right direction to regularly monitor Cygnus X-3 for the entire period of the outburst. The source was also monitored by the steerable telescope aboard Copernicus. But the X-ray data showed no corresponding increase in X-ray emission from Cygnus X-3.

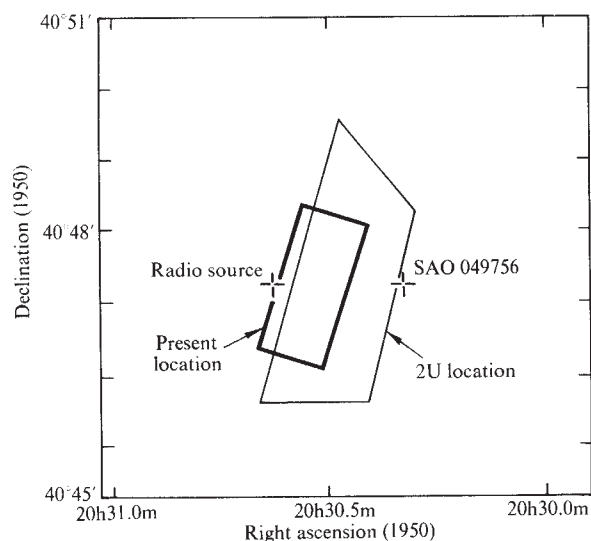


Fig. 1 Position of Cyg X-3. Heavy line defines region in which there is 90% probability of finding the source. From Parsignault *et al.*, *Nature Physical Science*, October 23 (1972).

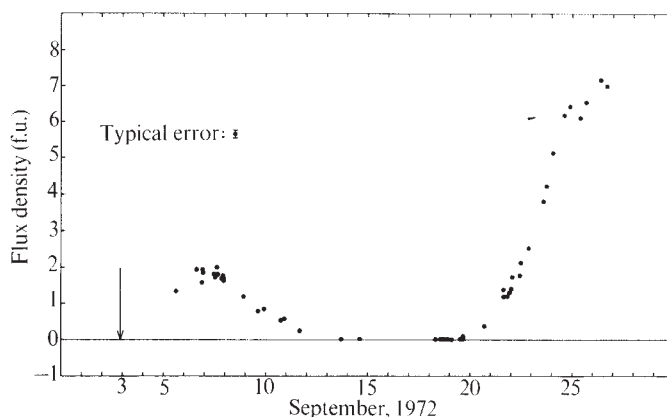


Fig. 2 The variation of the flux density at 408 MHz during September 1972. The time of the first reported detection on September 2 is shown. From Anderson *et al.*, *Nature Physical Science*, October 23 (1972).

The absence of a corresponding outburst at X-ray wavelengths is no longer surprising in terms of present understanding of the physical nature of the radio outburst. Even before the X-ray results were known the behaviour of the radio emission revealed characteristics of the source which were totally different from those assumed on the basis of observations made on the first night. The spectrum changed rapidly to that characteristic of a non-thermal source. In addition, the radio emission was found to be $\sim 4\%$ linearly polarized at 10.5 GHz, also indicative of a non-thermal source. Shortly after the flux density started to decay at high frequencies the flux density was still climbing at the lower frequencies. The consequent delay in the occurrence of the maximum flux density at the lowest frequency of 0.4 GHz, found by the Manchester group, was fully five days after the time of the maximum reported by the Canadians.

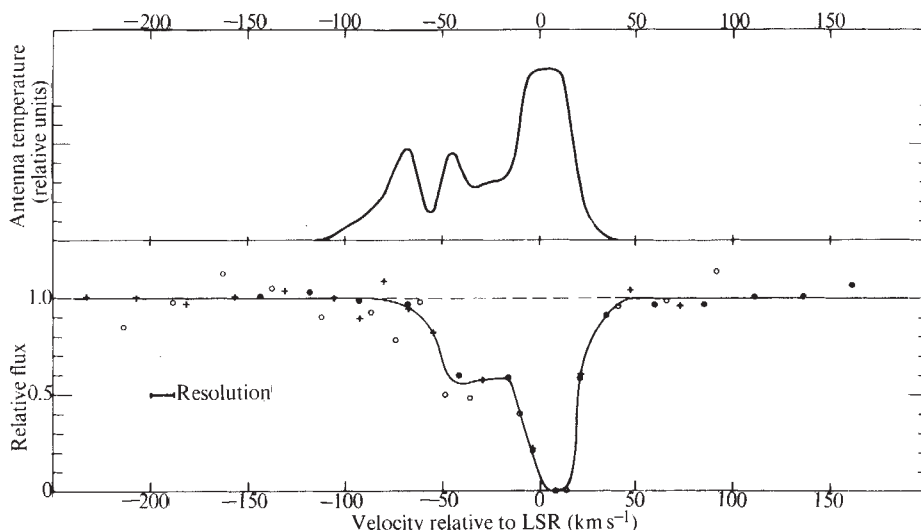
The picture that has emerged from combining data at many frequencies is that of an expanding cloud containing relativistic particles (electrons and protons moving at speeds close to that of light) and a magnetic field. In this model the radio emission arises from synchrotron radiation produced by the interaction of the relativistic electrons with the magnetic field.

Approximately two weeks after the first outburst a second outburst almost as strong as the first was reported, this time by the group at NRAO². This event later turned out to be a series of outbursts following in fairly rapid succession. As of October 6 a total of four outbursts have been detected and the new base level for the source flux density seems to be approximately 100 times its value before September 2.

The best estimate of the distance to the source, of between 8 and 11 k.p.c., has been obtained by French radio astronomers by observing absorption in the 21 cm hydrogen line produced by clouds of neutral hydrogen in the milky way between ourselves and Cygnus X-3. This distance places the source near the edge of the Galaxy and behind a large amount of interstellar dust. The extinction produced by this dust may explain why Cygnus X-3 has not been detected optically.

The only known sources of relativistic particles in the Galaxy are supernovae and pulsars, objects which astronomers believe may be related. Because radio astronomers have not yet observed the early stages of a supernova, no direct com-

Fig. 3 21 cm absorption spectrum for Cyg X-3 (bottom) compared with 21 cm emission profile from vicinity of the source (top). From Lauque *et al.*, *Nature Physical Science*, October 23 (1972)



parison with the outbursts from Cygnus X-3 is possible. There is also no evidence for periodicity in the radio emission which might be expected from a pulsar related phenomenon. But in a recent IAU circular³ the observation of a 4.8 h periodicity in the X-ray output of Cygnus X-3 has been reported, and this has also been found by the Copernicus experiment reported in next week's *Nature Physical Science*. Optical searches for a counterpart to Cygnus X-3 have proved unsuccessful; but that the source has been identified at infrared frequencies by a team using the 200-inch reflector at Mt Palomar.

It seems unlikely that Cygnus X-3 is unique with respect to

its radioactivity and it may therefore represent only one member of a large number of objects that exhibit highly variable radio emission, and which may have been overlooked in classical radio surveys.

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Discovery of Giant Radio Outburst from Cygnus X-3

P. C. GREGORY & P. P. KRONBERG

Scarborough College

E. R. SEAQUIST

David Dunlap Observatory, University of Toronto, Richmond Hill, Ontario

V. A. HUGHES, A. WOODSWORTH, M. R. VINER & D. RETALLACK

Astronomy Group, Queen's University, Kingston, Ontario

A giant radio outburst was observed from the X-ray source Cygnus X-3 on September 2, 1972. Flux density measurements were obtained at 10,522, 6,630 and 3,240 MHz as well as linear polarization measurements at 10,522 MHz. The evidence suggests a model of synchrotron radiation from an expanding cloud of relativistic particles. A computed upper limit for the source distance of ≤ 400 kpc implies that the source of the radio emission is galactic.

We have recently observed a 220-fold increase in radio emission from the X-ray source Cygnus X-3. The unprecedented characteristics of this outburst make it unique among radio astronomical phenomena. Here, we present details of the flux changes observed at 10,522, 6,630 and 3,240 MHz, as well as linear polarization measurements made at 10,522 MHz during the occurrence of this outburst. A theoretical discussion of the origin of the radio emission is given separately¹.

Cygnus X-3 is known as a weak but highly variable radio source². Two days before the outburst, the flux of Cygnus X-3 at 8,085 MHz⁴ was in the range 0.01 to 0.04 f.u. (1 f.u. = 10^{-26} W m⁻² Hz⁻¹). On September 2, at 2200 UT, we recorded a flux density of 21 f.u. at 10,522 MHz. This initial observation of the Cygnus X-3 outburst was made using the National Research Council 46 m Telescope at the Algonquin Radio Observatory in Ontario, Canada. Two hours later the flux density increased to a maximum of 22.5 f.u. and then decayed

during the course of the following week to its usual low level (Fig. 1).

Flux Density Measurements

The telescope was operated at 10,522 MHz with a beam switching facility using a reference beam 8 arc min below the main beam. Both beams were linearly polarized with the polarization of the main beam orthogonal to that of the reference beam for measurements of total intensity. A "nodding" technique was used whereby the source was placed first in the reference beam for 20 s, then in the main beam for 20 s, and then back into the reference beam. After each ref-main-ref beam observation the source amplitude was calculated using an on-line computer. Each set of 16 such observations covering a 20 min period was then averaged and normalized by comparison with the signal from a standard noise source. The noise source was calibrated daily with respect to 3C 123 using the same nodding technique. We assumed a flux density of 7.92 f.u. for 3C 123 at this frequency.

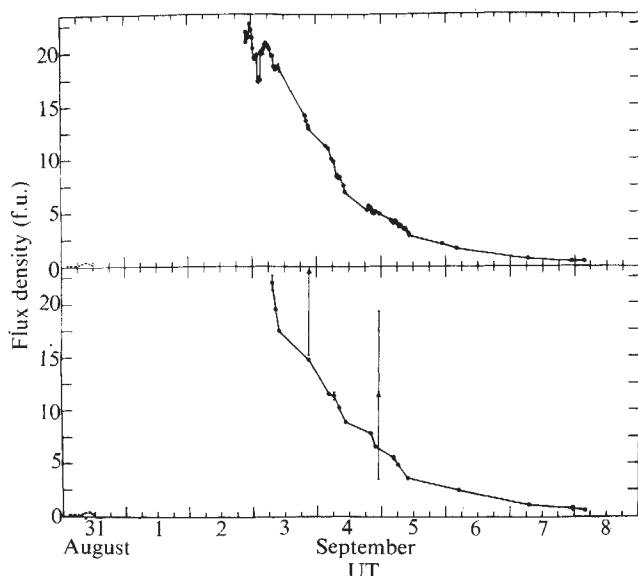


Fig. 1 Observed variations in the flux density of Cygnus X-3. Upper graph, observations at 10,522 MHz. Lower graph: ●, 6,630 MHz; ▲, 3,240 MHz.

Using the nodding technique three beam positions in the sky are involved; one is centred on the source, the other two are spaced 8 arc min away on either side. Because the 46 m telescope is alt-azimuth mounted the parallactic angle changes with the source hour angle and the two outer beam positions rotate about the source relative to the sky. If there were no confusing sources within this circle the measured source flux density would be constant. From the variation in measured flux density with hour angle it is possible to determine the effect of confusion, and we have carried out such an analysis using data from a set of observations of Cygnus X-3 obtained from July 2 to July 6, 1972, during which times its flux density remained in the range of 0.03 to 0.5 f.u. The resultant curve of confusion correction versus hour angle has been applied to the data here. The maximum correction for confusion at 10,522 MHz was 0.11 ± 0.03 f.u.

Seventeen observations at 6,630 MHz and two at 3,240 MHz were made. The 6,630 MHz measurements were obtained with a receiver located at the Gregorian focus of the 46 m telescope; the 3,240 MHz measurements were made using the 10 m equatorially mounted telescope of the Algonquin Radic

Observatory. In both cases a series of on-off observations were made. At 6,630 MHz the off-source position was 8 arc min from the source position, the same as the beam separation for the 10,522 MHz observations. We estimate that the uncertainty in flux density at 6,630 MHz due to confusion is ≤ 0.15 f.u. and no correction has been made. At 3,240 MHz the effect of confusion due to other sources in the Cygnus X complex was removed by observing the same region after the decay of the outbursts and then subtracting this result from the measurements obtained during the outburst.

Table 1 Telescope and Receiver Parameters

Telescope	Centre frequency (MHz)	Half-power beamwidth (arc min)	Receiver system temperature (K)	Receiver bandwidth (MHz)	Switching
46 m	10,522	2.8	180	100	Main beam—reference beam
46 m	6,630	4.5	450	210	Cold load
10 m	3,240	40.0	900	475	Cold load

The calibration sources used for observations at 6,630 and 3,240 MHz were 3C 123 (6,630 MHz flux density = 12.55 f.u.) and 3C 274 (3,240 MHz flux density = 100 f.u.). Table 1 gives the telescope and receiver parameters at the three frequencies used. The coordinates³ used for Cygnus X-3 were

$$\alpha (1950) = 20 \text{ h } 30 \text{ min } 37.63 \text{ s}$$

$$\delta (1950) = +40 \text{ arc degree } 47 \text{ arc min } 12.5 \text{ arc s}$$

The flux densities measured at 10,522 and 6,630 MHz are listed in Table 2 together with values of the spectral index (α) derived from the relationship

$$\log (S_{10522}/S_{6630}) = \alpha \log (10522/6630)$$

The two flux density measurements obtained at 3,240 MHz were

$$\text{Sept. 3 2030 UT; } S_{3240} = 23.1 \pm 7.8 \text{ f.u.}$$

$$\text{Sept. 5 2250 UT; } S_{3240} = 11.6 \pm 7.8 \text{ f.u.}$$

The increased flux density observed on September 2 over the level observed on August 31 by Hjellming and Balick⁴ and its subsequent decay is shown in Fig. 1. We estimate the peak of the outburst at 10,522 MHz to have occurred at September 3.0 ± 0.25 days. The measurements made on August 31 make it possible to set an upper limit on the rise time, and hence the source size, at about one light day (3×10^{15} cm). At the peak of the outburst significant secondary variability in flux density was also observed; for example, the 3 f.u. increase between 0330 and 0530 UT on September 3. This suggests that the source size could be even less than a few light hours.

Fig. 2 shows the behaviour of the spectral index during the outburst. A gradual evolution from a flat spectrum ($\alpha \approx 0$) near the peak of the outburst to a fairly constant value of 0.68 ± 0.15 is apparent. There is also evidence for significant short term variations in α which may reflect the occurrence of secondary activity near the peak of the outburst.

Polarization Measurements

Linear polarization measurements were obtained using a rotating polarimeter. One measurement consisted of a rotation on-source (lasting 4 min) followed by a rotation off-source. An on-line computer subtracted the on- and off-source samples

corresponding to the same polarizer angle and computed the per cent polarization and position angle. The instrumental polarization obtained from daily measurements of the unpolarized source⁵ W51-8 was subtracted from each Cygnus X-3 measurement to give the final percentages and position angles. Measurements made on four different occasions during

radio sources. But the magnitude of the flux density change observed from Cygnus X-3 distinguishes it from any previously known radio event. The nature of the source suggested by the flux density, spectral index and polarization data is that of an expanding cloud containing a magnetic field and relativistic particles radiating by the synchrotron mechanism. The

Table 2 Cygnus X-3 Flux Density Measurements

Date	UT	Flux density (10^{-26} W m $^{-2}$ Hz $^{-1}$)		Spectral index α	Date	UT	Flux density (10^{-26} W m $^{-2}$ Hz $^{-1}$)		Spectral index
		10,522 MHz	6,630 MHz				10,522 MHz	6,630 MHz	
September 2, 1972	2214	$20.92 \pm 2\%$			September 4	0754	8.483		
	2235	21.38				0814		10.29	-0.44 ± 0.08
	2255	21.55				0822	8.335		
	2318	21.36				0838	8.187		
	2341	22.67				0855	8.292		
September 3	0003	$22.08 \pm 2\%$				1015	7.584		
	0024	21.32				1038		8.93	-0.45 ± 0.08
	0045	20.37				1050	6.932		
	0107	19.45				1854	5.236		
	0130	19.43				1935	5.574		
	0221	19.79				2005		7.854	-0.76 ± 0.08
	0243	17.30				2022	5.494		
	0303	17.47				2043	5.265		
	0326	17.42				2103	4.962		
	0348	20.03				2124	4.898		
	0410	19.89				2150		6.649	-0.63 ± 0.07
	0432	20.24				2207	5.063		
	0452	20.30				2328	4.907		
	0529	20.88			September 5	0347	$4.318 \pm 2\%$		
	0550	20.73				0409	4.224		
	0611	20.68				0433		$5.619 \pm 3\%$	-0.65 ± 0.09
	0637	20.56				0456	4.109		
	0658	20.24				0518	4.162		
	0730		$21.99 \pm 3\%$	-0.21 ± 0.08		0539	4.221		
	0747	19.71				0603		4.965	-0.43 ± 0.08
	0805	19.62				0626	3.931		
	0826	18.69				0657	3.742		
	0848		19.61	-0.12 ± 0.07		0718	3.789		
	0903	18.35				0839	3.547		
	0924	18.62				0901	3.565		
	0944	18.60				0921	3.459		
	1000		17.61	$+0.14 \pm 0.05$		0945		3.712	-0.27 ± 0.10
	1020	18.52				1006	3.099		
	1953	14.16				1027	2.901		
	2010	14.07				2243	2.116		
	2029	13.63			September 6	0417	$1.735 \pm 3\%$		
	2052		14.84	-0.22 ± 0.07		0455		$2.584 \pm 3\%$	-0.86 ± 0.1
	2114	13.14			September 7	0647	$0.723 \pm 6\%$		
	2139	12.80				0702		$1.074 \pm 6.8\%$	-0.86 ± 0.2
September 4	0349	$11.25 \pm 2\%$				2255	$0.480 \pm 10\%$		
	0417		$11.63 \pm 3\%$	-0.092 ± 0.08		2320		$0.758 \pm 11\%$	-0.99 ± 0.3
	0431	11.05			September 8	0330	$0.447 \pm 8\%$		
	0557	10.05				0412		$0.574 \pm 6.4\%$	-0.54 ± 0.2
	0618		11.39	-0.30 ± 0.08					
	0633	9.765							

the outburst are given in Table 3. All indicate significant polarization. Both the percentage polarization and position angle were found to evolve during the course of the outburst.

Implications

In many respects the giant outburst from Cygnus X-3 is analogous to the outbursts observed in variable extragalactic

evolution in spectral index might reflect a corresponding evolution in the energy spectrum of the radiating particles and/or changes in the optical depth of the source. The marked increase in polarization during the decay is also consistent with a model of synchrotron radiation from a source in which the optical depth was evolving from thick to thin. According to this model we would expect the flux density to reach a maximum during this evolution in optical depth. If we thus

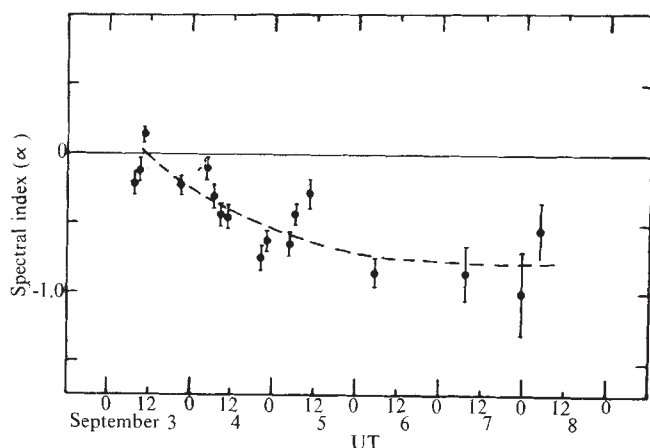


Fig. 2 Measurements of the spectral index of Cygnus X-3 between 6,630 MHz and 10,522 MHz during the course of the outburst ($S=f^{\alpha}$).

assume that the flattening in flux density observed near the beginning of the 10,522 MHz observations is explained by synchrotron self-absorption, the angular size (θ), distance (D) and brightness temperature (T) can be estimated using the relationship

$$S \approx 5.3 \times 10^{-4} B_{\text{gauss}}^{-1/2} \theta_s^2 f_{\text{MHz}}^{2.5} \text{ f.u.}$$

where S is the maximum flux density observed at the frequency f .

$$\theta \text{ (arc s)} \approx 1.9 \times 10^{-3} B^{1/4}$$

$$D \text{ (kpc)} \approx 3.6 \times 10^{-14} L B^{-1/4}$$

$$TK \approx 7.3 \times 10^{10} B^{-1/2}$$

These expressions depend only on the magnetic field (B) in gauss and on the source size (L) in cm.

A lower limit on B can be set from the argument that $T \leq 10^{12}$ K in order to avoid catastrophic inverse Compton losses. Setting $T = 10^{12}$ K we obtain $B \geq 5.3 \times 10^{-3}$ gauss so that

$$\theta \geq 5.1 \times 10^{-4} \text{ arc s}$$

and assuming $L \leq 1$ light day. Thus $D \leq 400$ kpc, a value which suggests that the source is within the Galaxy.

Table 3 Linear Polarization Data for Cygnus X-3 at 10.5 GHz

Date	Universal time	Linear polarization (%)	Position angle (degrees)
September 3, 1972	0510	1.65 ± 0.11	142 ± 3
September 4, 1972	1915	2.51 ± 0.93	114 ± 3
September 4, 1972	2221	3.46 ± 0.68	107 ± 4.5
September 4, 1972	2313	4.96 ± 0.53	102 ± 3

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Unusual Radio Events in Cygnus X-3

R. M. HJELLMING & B. BALICK

National Radio Astronomy Observatory, Green Bank, West Virginia

Observations made at Green Bank confirm the findings of Gregory *et al.*

ON September 2, 1972, the radio emission from a source identified with the X-ray source Cyg X-3 (refs. 1, 2) was seen at the unprecedented level of approximately 20 f.u., after having increased its flux by three orders of magnitude in less than two days. Here we report observations of Cyg X-3 at 2,695 and 8,085 MHz beginning shortly after the initial discovery of the unusual flux level by Gregory *et al.*, and extending for the next ten days. In addition, we present observations of

Cyg X-3 at these frequencies during the three months preceding the great flare, including data taken two days before. We also describe simultaneous observations at these frequencies and at 31.5 GHz which indicate that events similar to the great flare may occur at weaker levels almost daily in Cyg X-3.

The flux densities S_{8085} and S_{2695} , and an empirical spectral index $\alpha_{2695}^{8085} = \log(S_{8085}/S_{2695})/\log(8085/2695)$, are presented in Table 1 for all the observations of Cyg X-3 made with the NRAO interferometer since May 13, 1972.

The 2,695 and 8,085 MHz observations of the great flare began at 2230 UT on September 2, 1972, and continued at various times through September 11. The measurements of S_{2695} , S_{8085} , and α_{2695}^{8085} obtained from August 31 to Sep-

tember 11 are plotted as a function of time in Fig. 1. The rise in flux at 2,695 MHz on September 2-3 is very rapid and well defined. By extrapolating backwards in time, we estimate that the event must have begun at roughly 0700 UT on September 2.

The surprisingly smooth variation of α_{2695}^{8085} makes the point in time where $S_{8085} \approx S_{2695}$ is easily identifiable. Because of

this and the clear decrease in S_{8085} , it is quite well established that S_{2695} must have peaked at about 16 f.u. at about 1200 UT on September 3. In addition, α_{2695}^{8085} decreased to a nearly constant level of -0.58 ± 0.05 after about September 6.

During all of the observations the source remained unresolved and within 0.5 arc s of the position previously reported^{1,2}. Thus the angular size remained less than 0.5 arc s

Table 1 Radio Observations of Cyg X-3 at 2,695 and 8,085 MHz

Date	UT	S_{8085}	S_{2695}	α_{2695}^{8085}	Date	UT	S_{8085}	S_{2695}	α_{2695}^{8085}		
May 13, 1972	0513	0.034 ± 0.008	0.037 ± 0.008	-0.26 ± 0.1	July 4, 1972	0143	0.067 ± 0.008	0.043	$+0.09 \pm 0.1$		
	0533	0.027	0.039			0209	0.116	0.042			
	0608	0.023	0.030			0244	0.113	0.073			
	0633	0.032	0.030			0308	0.091	0.074			
	0708	0.031	0.049			0344	0.079	0.078			
	0733	0.021	0.033			0408	0.074	0.061			
	0808	0.028	0.043			0444	0.087	0.046			
	0833	0.020	0.030			0508	0.092	0.075			
June 21, 1972	0837	0.038 ± 0.008	0.097 ± 0.008	-0.53 ± 0.15	0543	0.063	0.068	$+0.01 \pm 0.1$			
	0934	0.060	0.097		0608	0.058	0.068				
	1034	0.063	0.089		0643	0.036	0.056				
	1134	0.044	0.085		0708	0.034	0.048				
June 22, 1972	0758	0.069 ± 0.008	0.078 ± 0.008	-0.11 ± 0.15	0743	0.038	0.050			$+0.01 \pm 0.1$	
	0855	0.063	0.084	-0.26	0911	0.110	0.091				
	0955	0.076	0.093	-0.18	1011	0.071	0.098				
	1055	0.106	0.100	$+0.05$	1111	0.043	0.055				
	1155	0.071	0.106	-0.36	1141	0.034	0.050				
	1254	0.051	0.092	-0.54 ± 0.15	1207	0.034	0.048				
June 24, 1972	0223	0.045 ± 0.008	0.066 ± 0.008	-0.33 ± 0.2	July 5, 1972	0140	0.065 ± 0.008	0.053 ± 0.008			$+0.01 \pm 0.1$
	0323	0.065	0.061	$+0.06$		0214	0.050	0.044			
	0423	0.067	0.080	-0.16		0240	0.065	0.055			
	0523	0.038	0.053	-0.30		0305	0.069	0.052			
	0623	0.038	0.042	-0.09		0339	0.048	0.064			
	0723	0.039	0.049	-0.21		0404	0.042	0.044			
	0823	0.045	0.053	-0.15		0439	0.041	0.040			
	0923	0.033	0.054	-0.45		0504	0.037	0.057			
	1023	0.035	0.050	-0.32		0539	0.055	0.048			
	1125	0.060	0.058	$+0.03$		0604	0.060	0.049			
	1224	0.065	0.079	$+0.18 \pm 0.2$		0639	0.045	0.053			
	June 25, 1972	0746	0.011 ± 0.008	0.027 ± 0.008		-0.75 ± 0.3	0704	0.050	0.054	$+0.17 \pm 0.2$	
0843		0.015	0.028	0739	0.042		0.064				
0943		0.009	0.025	0906	0.114		0.075				
1043		0.072	0.029	0937	0.059		0.066				
1143		0.069	0.055	1006	0.060		0.078				
1242		0.032	0.062	1031	0.062 ± 0.012		0.073 ± 0.012				
July 2, 1972	0159	0.022 ± 0.008	0.034 ± 0.008	-0.05 ± 0.1	1043	0.099	0.082	-0.28 ± 0.15			
	0218	0.024	0.028		1057	0.087	0.071				
	0253	0.023	0.036		1109	0.109	0.058				
	0318	0.026	0.031		1132	0.181	0.080				
	0353	0.018	0.034		1144	0.244	0.076				
	0417	0.014	0.027		1157	0.274	0.101				
	0453	0.027	0.019		1209	0.353	0.091				
	0516	0.026	0.030		July 6, 1972	0136	0.153 ± 0.008		0.209 ± 0.008	-0.20 ± 0.15	
	0551	0.027	0.026			0201	0.128		0.159		
	0616	0.029	0.028			0236	0.102		0.157		
	0652	0.037	0.030			0301	0.070	0.118			
	0716	0.043	0.031			0336	0.060	0.111			
	0851	0.022	0.035			0400	0.054	0.091			
	0919	0.049	0.040			0435	0.129	0.075			
	1019	0.082	0.057			0500	0.129	0.092			
1119	0.060	0.070	0535	0.069		0.082					
1219	0.069	0.078	0600	0.054		0.077					
July 3, 1972	0146	0.058 ± 0.008	0.099 ± 0.008	-0.22 ± 0.1		0635	0.034	0.052	-0.39 ± 0.15		
	0212	0.060	0.077			0700	0.030	0.053			
	0247	0.056	0.090			0735	0.025	0.051			
	0312	0.060	0.068			0800	0.029	0.035			
	0347	0.050	0.083			0835	0.029	0.053			
	0412	0.051	0.055		0859	0.020	0.047				
	0447	0.056	0.043		0934	0.022	0.044				
	0512	0.054	0.064		0959	0.016	0.032				
	0547	0.030	0.052		1034	0.026	0.037				
	0612	0.039	0.056		1059	0.025	0.030				
	0647	0.038	0.058		1134	0.032	0.028				
	0712	0.062	0.058		July 7, 1972	0119-1142	0.027 ± 0.004	0.023 ± 0.004	$+0.12 \pm 0.05$		
	0747	0.094	0.077			July 12, 1972	0811	0.064 ± 0.008		0.074 ± 0.008	-0.03 ± 0.1
	0916	0.063	0.084				0835	0.080		0.075	
	1015	0.046	0.092				1018	0.070 ± 0.02		0.082 ± 0.02	
	1115	0.035	0.059				1036	0.079		0.074	
	1215	0.069	0.060				1111	0.082		0.094	
							1136	0.097		0.102	

Table 1—continued

Date	UT	S_{8085}	S_{2695}	α_{2695}^{8085}	Date	UT	S_{8085}	S_{2695}	α_{2695}^{8085}	
July 13, 1972	0807	0.045 ± 0.008	0.030 ± 0.008	$+0.37 \pm 0.3$	Sept. 2/3, 1972	0108	19.7	10.9	$+0.54$	
	0832	0.097	0.039	$+0.80 \pm 0.2$		0144	19.0	11.1	$+0.49$	
	0910	0.044 ± 0.02	0.055 ± 0.02	-0.2 ± 0.2		0208	19.0	11.4	$+0.46$	
	0932	0.100	0.055	$+0.54 \pm 0.2$		0244	18.2	11.4	$+0.43$	
	1012	0.175	0.046	$+1.2 \pm 0.2$		0308	18.6	11.9	$+0.41$	
	1035	0.151	0.078	$+0.60 \pm 0.2$		0343	18.2	12.3	$+0.36$	
	1103	0.203	0.115	$+0.52 \pm 0.2$		0408	18.4	12.5	$+0.35$	
	0443	19.6	13.3	$+0.35$						
July 14, 1972	0803	0.086 ± 0.008	0.064 ± 0.008	$+0.27 \pm 0.2$	0508	20.4	13.7	$+0.36$		
	0828	0.067	0.070	-0.04	0543	19.4	14.1	$+0.29$		
	0906	0.049	0.066	-0.27	0608	19.9	14.2	$+0.31$		
	0928	0.040	0.064	-0.43 ± 0.2	0644	19.3	14.7	$+0.25$		
	1005	0.024 ± 0.02	0.054 ± 0.02	-0.42 ± 0.2	0709	19.3	15.0	$+0.23$		
	1028	0.032	0.041		0744	20.2	15.0	$+0.27$		
	1103	0.022	0.036		0809	18.6	15.4	$+0.17$		
	1128	0.024	0.031							
July 16, 1972	0756	0.092 ± 0.008	0.089	$+0.03 \pm 0.1$	Sept. 3/4, 1972	2141	11.6 ± 0.5	14.3 ± 0.4	-0.19 ± 0.05	
	0820	0.091	0.083	$+0.08$		2240	11.6	13.7	-0.15	
	0858	0.139	0.080	$+0.50$		2341	11.6	13.6	-0.14	
	0920	0.206	0.082	$+0.84$		0041	11.2	13.0	-0.14	
	0958	0.178	0.119	$+0.37$		0140	10.2	12.4	-0.18	
	1020	0.127	0.122	$+0.04$		0240	10.7	12.3	-0.13	
	1055	0.111	0.109	$+0.02$		0340	10.8	12.2	-0.11	
	1121	0.102 ± 0.008	0.095	$+0.06 \pm 0.1$		0440	10.6	12.2	-0.13	
July 17, 1972	0939–1128	0.015 ± 0.005	0.016 ± 0.005	-0.07 ± 0.15	0540	10.7	12.4	-0.13		
July 18, 1972	0935–1124	0.033 ± 0.005	0.047 ± 0.005	-0.32 ± 0.1	0641	10.1	12.3	-0.18		
					0741	10.4	12.0	-0.13		
Aug. 30/31, 1972	2208	0.015 ± 0.005	0.016 ± 0.005	0.06 ± 0.2	Sept. 5, 1972	0235	5.0 ± 0.2	7.3 ± 0.2	-0.34 ± 0.05	
	2308	0.015	0.012		Sept. 6, 1972	0433	2.0 ± 0.1	3.3 ± 0.1	-0.46 ± 0.05	
	0008	0.015	0.016	Sept. 7, 1972		0147	0.95 ± 0.07	1.8 ± 0.1	-0.58 ± 0.05	
	0107	0.011	0.009		0.79 $\pm$ 0.07	1.4 ± 0.1	-0.52 ± 0.05			
	0207	0.014	0.009	Sept. 7/8, 1972	2331	0.45 ± 0.03	0.91 ± 0.05	-0.64 ± 0.05		
	0307	0.011	0.011		0.45 $\pm$ 0.03	0.85 ± 0.05	-0.59 ± 0.05			
	0407	0.016	0.008		0.40 $\pm$ 0.03	0.80 ± 0.05	-0.63 ± 0.05			
	0507	0.035	0.013		0.26 $\pm$ 0.02	0.52 ± 0.03	-0.63 ± 0.05			
	0607	0.038	0.013	1.0	Sept. 9, 1972	0318				
	0706	0.031	0.017	0.55						
	0806	0.020	0.022	-0.09	Sept. 10, 1972	0722	0.16 ± 0.01	0.29 ± 0.01	-0.54 ± 0.05	
Sept. 2/3, 1972	2250	18.4 ± 0.9	9.4 ± 0.4	$+0.61 \pm 0.05$		Sept. 11, 1972	0337	0.10 ± 0.01	0.19 ± 0.01	-0.57 ± 0.05
	2308	19.0	9.7	$+0.61$						
	2344	19.9	10.2	$+0.61$						
	0008	18.8	9.6	$+0.61$						
0044	19.0	10.5	$+0.54$							

so the lower limits to the brightness temperature are 1.5×10^7 and 2.0×10^6 K at 2,695 and 8,085 MHz, respectively, when the source was strongest.

Preliminary analysis of the data indicates the polarization was less than 1% at both frequencies during the first two days the great flare was observed. More detailed analysis is in progress.

The behaviour of the flux densities and spectral index in Fig. 1 strongly suggests that the event is the result of synchrotron radiation produced by the ejection of an expanding relativistic plasma from what is presumably the X-ray source. Initially the radio source is at least partially self-absorbed, especially at lower frequencies. After September 3 it clearly becomes non-thermal, and after about September 4 it appears optically thin. Before September 6 a combination of self-absorption and evolution of the electron energy distribution affects the empirical spectral index.

From the standard formulae⁴ for a self-absorbed synchrotron source, we can relate the angular diameter (θ in arc s) to the magnetic field strength (B in gauss), the observing frequency at maximum (ν_m in GHz), and the extrapolated flux at maximum in absence of absorption (S_m in f.u.) by

$$\theta \approx 0.006 B^{1/4} S_m^{1/2} \nu_m^{-5/4} \approx 0.008 B^{1/4} \quad (1)$$

where a value for S_m of 21 f.u. at 2,695 MHz has been adopted. Because $\theta < 0.5$ arc s, it follows that $B < 2 \times 10^7$ gauss at this time. A more interesting limit can be obtained from the fact that inverse Compton losses will prevent the brightness temperature from exceeding 10^{12} K. Therefore, $\theta > 0.002$ arc s

on September 3; this means $B > 4 \times 10^{-3}$ gauss. These limits are based on the assumption that the radiation is not coherent. We note that with such a large lower limit on θ at such an early stage, there is a possibility that the expanding remnant of the great flare could eventually become resolvable. In that case, we could estimate the value of B on September 3 by assuming an expansion model and an initial time for the event.

The interpretation of this event as arising from a transient synchrotron radiation source suggests that when the source becomes optically thin, and the spectral index is constant as it appears to be after September 6, then the observed fluxes could be fitted by a power law of the form $S_\nu \propto \nu^\alpha (t - t_0)^\beta$, where α is the usual spectral index. Indeed, a least squares fit to the data after September 6 gives

$$S_\nu = (743 \pm 101) \left(\frac{t - t_0}{\text{days}} \right)^{-3.5 \pm 0.07} \left(\frac{\nu}{\text{GHz}} \right)^{-0.58 \pm 0.05} \quad (2)$$

We have taken t_0 to correspond to 0700 UT on September 2; if this choice were very much in error, the fit would not be as good. The spectral index is similar to that often observed in non-thermal sources whose electron energy (E) distribution is proportional to $E^{-\gamma}$, where $\gamma = 1 - 2\alpha \approx 2.2 \pm 0.1$. Before September 6 α , and hence γ , shows clear evidence of evolution. Before September 4 the empirically determined α is strongly affected by self-absorption. According to Kellermann⁴, a synchrotron source, with a single injection event, evolves from a power law with spectral index α_0 to one with a spectral index $(4\alpha_0 - 3)/3$; therefore this model would give $\alpha_0 \approx 0.32$. With

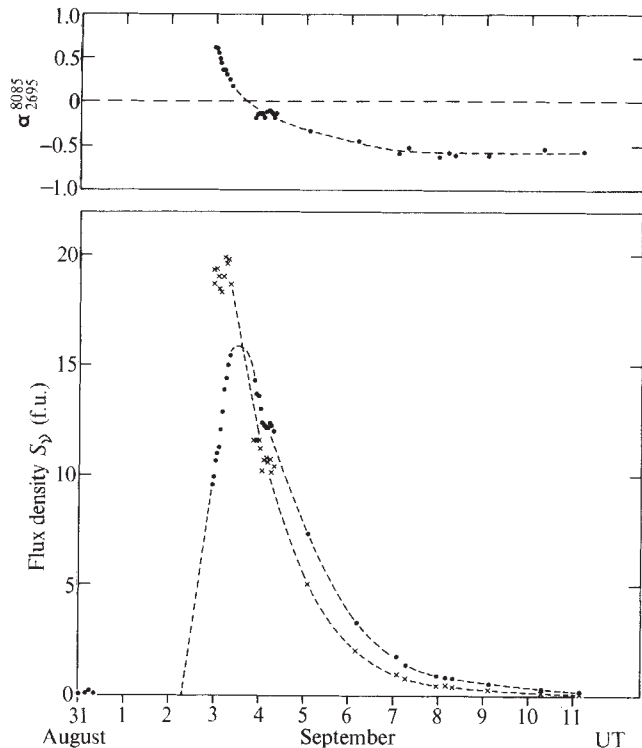


Fig. 1 The radio flux densities at 2,695 (●) and 8,085 MHz (×), and an empirical spectral index for the radio counterpart of Cyg X-3 are plotted as a function of UT for the period August 31 to September 11, 1972.

observations at millimetre wavelengths are of great interest. During July 2–6, 1972, simultaneous observations at 31.5 (with the NRAO 36-foot antenna), 8,085 and 2,695 GHz revealed a number of events which were significantly stronger at the highest frequency. Data for these three frequencies taken on July 4, 5 and 6 are shown in Fig. 2. Only marginally significant events were seen at 31.5 GHz on July 4 (and one on July 2, 3); however, on July 5 a clear flare event was detected at all three frequencies, with the highest frequency showing the greatest intensity and earliest peaking. This behaviour is just as expected for a synchrotron source which is initially self-absorbed. Further, when the source was observed again about 12 h later, Cyg X-3 appeared as a decaying non-thermal source (Fig. 2). In all of its essential characteristics this event is similar to, but a factor of 100 times weaker than, the great flare beginning on September 2.

Equally interesting are the three brief but intense flares at 31.5 GHz observed on July 6 without any accompanying events at the other frequencies. Because $S_{31.5} \sim 1$ f.u. at the peak of all three events, $\alpha_{8085}^{31.5}$ was about 2.5, as expected for an optically thick synchrotron source. It is noteworthy that for all of the data available on Cyg X-3, the radio flux is much greater at higher frequencies only when the source is flaring, whereas non-thermal spectral indices are seen only when the source strength is decreasing or remaining relatively constant.

Out of the fifty hours on five different days when Cyg X-3 has been observed at millimetre wavelengths, at least four brief but intense events were observed. This information, the spectral indices at those times, and the frequent variability of Cyg X-3 suggest that events which are essentially miniatures of the great flare of September 2 are probably occurring almost on a daily basis. This means such interesting events can be most easily studied by concentrating on the higher frequencies.

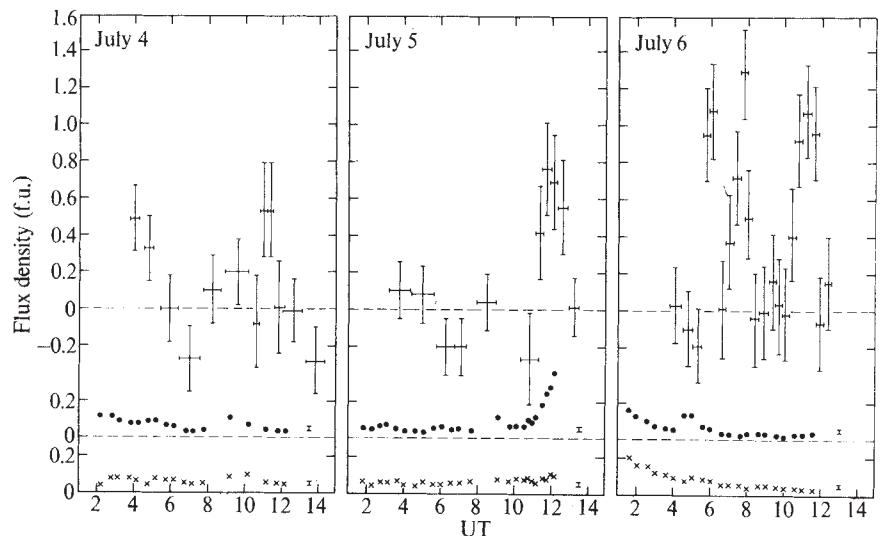


Fig. 2 The radio flux densities of Cyg X-3 at 31,490 MHz (×), 8,085 MHz (●), and 2,695 MHz (×) plotted as a function of UT for observations during July 4–6, 1972.

continuous re-supply the final state would have a spectral index ($\alpha_0 - 0.5$), in which case $\alpha_0 \sim -0.08$.

Observations of this event at many frequencies will, for the first time, provide a great deal of information with which to test theories of expanding synchrotron sources. The association of cosmic ray ejection with this object provides another possible source of cosmic rays in the galaxy.

The extensive observations of Cyg X-3 at 2,695 and 8,085 MHz presented in Table 1 include observations just two days before the event. On August 31 the radio source was at the lowest levels yet observed at these frequencies (0.01 f.u. at both frequencies at 0107 UT on August 31). The minor flare a few hours after this low state is typical of many seen previously in Cyg X-3, as well as in other flaring radio stars such as β Persei (Algol)^{5,6}.

Because a rising flare is always strongest at higher frequencies,

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LETTERS TO NATURE

PHYSICAL SCIENCES

Cosmic Effects of Gravitational Waves

KAFKA¹ has considered the possibility that gravitational radiation is of primordial origin. He treated this radiation in the high frequency limit² and used the Friedmann equations to determine its effect on the evolution of the cosmos. Using Weber's intensity measurements, he found that the lifetime of the universe would be shortened to 3.3×10^9 yr and concluded that the observed wave pulses cannot be remnants of the big bang.

Since our galaxy is typical, if the observed radiation is of galactic rather than cosmological origin then it must be assumed that all galaxies are radiating gravitationally at roughly the same rate. This leads to a large total amount of radiation which should be included in the gravitational dynamics of the expanding universe. We have constructed a model to incorporate the galactic emission of gravitational waves in the later phases of cosmological evolution and have examined in detail the resultant shortening of the lifetime of the universe.

We describe the gravitational radiation in terms of two parameters f_0 and n . The first parameter, f_0 , gives the present fractional conversion rate of matter into gravitational waves

$$f_0 = \left. \frac{d}{dt} \ln(\epsilon R^3) \right|_{t=t_0}$$

where ϵ is the energy density of matter. Weber's data imply a conversion rate of $10^3 M_\odot$ per yr (ref. 3), although larger values have been suggested. For our galaxy, $10^3 M_\odot$ per yr corresponds to the value $f_0 = 10^{-8}$ per yr. The second parameter, n , determines the profile of the mass loss rate as a function of time. More precisely, we assume that the energy density of gravitational radiation is related to the expansion of the universe according to

$$\rho = \rho_0 (R/R_0)^{n-4}$$

We consider only values $n > 2$, for which the radiation energy

$$U \equiv \rho R^3 = U_0 (R/R_0)^{n-1}$$

vanishes at the time of the big bang. The time dependence of the radiation is determined from its radius dependence *via* the Friedmann equations. Following the big bang, more and more matter is converted into gravitational radiation. For increasing values of the parameter n , this conversion is shifted closer towards the present. In the limit $n \rightarrow \infty$, all the gravitational radiation would be emitted in the last instant. For the value $n=10$, 90% of the radiation would be emitted in the last $0.23 H_0^{-1}$ yr, where H_0 is the Hubble constant; that is subsequent to the formation of the Earth. This corresponds with typical time scales for galactic evolution. In the absence of any satisfactory models for gravitational wave sources, the value $n=10$ gives a plausible time scale for the development of radiative processes within the galaxy.

From the Friedmann equations modified for the continual

burning of matter into gravitational radiation, we find the lifetime of the universe to be

$$t_0 = H_0^{-1} \Omega^{-1} (1 - nB)^{\frac{1}{n-1}} \int_0^1 [1 - Ax - Bx^{n-1}]^{-\frac{1}{n-1}} x^{\frac{1}{n-1}} dx$$

where

$$a = [nH_0/f_0 + n/(n-1)]^{-1}$$

$$B = a/(n-1)$$

$$A = \Omega^{-1}[(\Omega-1)(1-B) + a]$$

and

$$\Omega = 8\pi G\epsilon_0/(3c^2 H_0^2)$$

The value $\Omega=1$ corresponds to the critical matter density necessary to close the universe if no gravitational radiation were present.

To evaluate the lifetime numerically we need two additional observational parameters: the present energy density ϵ_0 and the Hubble constant H_0 . If we assume $H_0^{-1} = 13 \times 10^9$ yr, as accepted in the past literature, $f_0 = 10^{-8}$ per yr and $n=10$, then 90% of the radiation was emitted within the past 4×10^9 yr. We find for various densities the following lifetimes:

$$\Omega = 10, t_0 = 1.3 \times 10^9 \text{ yr}$$

$$\Omega = 1, t_0 = 3.6 \times 10^9 \text{ yr}$$

$$\Omega = 0.1, t_0 = 7.7 \times 10^9 \text{ yr}$$

$$\Omega = 0.05, t_0 = 8.9 \times 10^9 \text{ yr}$$

The cases $\Omega=10$ and $\Omega=1$ are inconsistent with the age of our galaxy, 7×10^9 yr, determined by uranium dating⁶. The cases $\Omega=0.1$ and $\Omega=0.05$ are inconsistent with the lower limit of 9×10^9 yr for the ages of population II star clusters⁷. This implies that the density of matter must be relatively small, $\Omega < 0.05$, to agree with Weber's measurements. Work by Sandage and Tammann (personal communication) points to a considerably longer Hubble time. In that case, the above numbers have to be modified but gravitational radiation with the intensity observed still shortens the calculated lifetime by a factor of order 2.

In addition to its effects on the time scale of the cosmos, this radiation has another important consequence. The present energy density of gravitational waves is a factor of order 20 larger than ϵ_0 so that relatively small matter densities lead to closed universes.

We conclude that gravitational radiation, even though it is not a remnant of the big bang, is of importance to cosmological models.

RICHARD A. ISAACSON

*Department of Physics,
Illinois Institute of Technology*

JEFFREY WINICOUR

*Aerospace Research Laboratories,
Wright-Patterson Air Force Base*

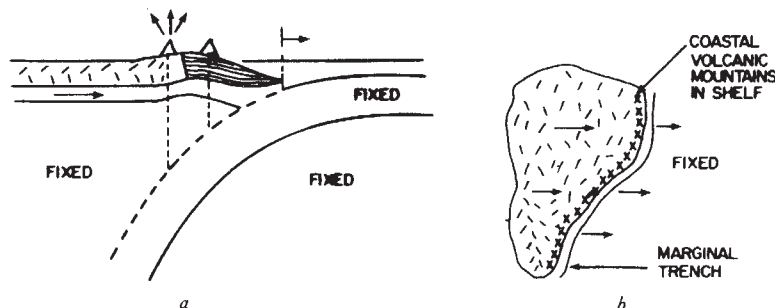
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Two Types of Mountain Building

ACCORDING to plate tectonic theory subduction zones form where two plates converge. Observation shows that these zones frequently form along the margins of continental plates where they override oceanic plates with Benioff zones dipping towards the continents. We consider the motion relative to the deeper mantle of two converging plates. Neglecting lateral shear components we find three cases possible. First, continental plate may be actively advancing and overriding an oceanic plate which is stationary relative to the mantle. Second, an oceanic plate may be actively advancing and passing beneath a stationary continental plate. Third, both plates may be in motion relative to the mantle, but if so their behaviour may be nearer to one or the other of the two previous cases.

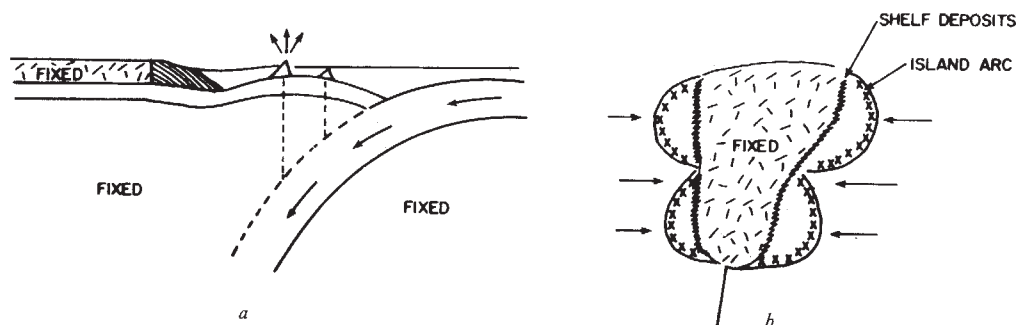
Fig. 1 *a*, Section showing continental plate advancing over an oceanic plate stationary on the mantle. The marginal trench is advancing and the shelf deposits are intruded and disturbed by volcanism arising from the Benioff zone (dashed line). *b*, Sketch plan of *a* indicating that orogeny can only affect the leading edge of the continent.



Attempts have been made to determine the absolute motions of some existing plates relative to the deeper mantle^{1,2}, using Wilson's³ proposal that convective plumes rise in the mantle and are fixed in location like such common diapiric structures as salt domes, pingos and volcanic pipes and Morgan's⁴ hypothesis that these plumes drive the lithospheric plates. For the case in which a continental plate is actively advancing, the subduction zone takes the form of coastal mountains as in Chile with a marginal trench which is pushed ahead of the continent⁵ (Fig. 1*a, b*), and for the case in which the oceanic plate is actively advancing, island arcs and trenches form off-shore as in East Asia with little disturbance of the continental coast⁶ (Fig. 2*a, b*).

In the first case it is evident that a marginal trench and coastal mountains can only form at any one time on a single side, that is the leading edge, of an advancing continental plate (Fig. 1*b*); the Andes range appears a good example.

Fig. 2 *a*, Section showing oceanic plate advancing and passing beneath a continental plate stationary over the mantle. The shelf deposits are not affected by the arc volcanism which forms a separate deposit. *b*, Sketch plan of *a* indicating that more than one moving oceanic plate may approach a fixed continent and produce orogeny on several sides at the same time.



in marginal seas and to the East of the Andes shows that this volcanism is confined to narrow belts extending a few hundred kilometres landwards from trenches.

Where orogeny has produced marginal trenches as in the Andes, Sierra Nevada and Coast Ranges of British Columbia, this volcanism rises close to the coast. Gilluly¹¹ and James¹² have discussed how the sedimentary deposits of the continental shelves have been intimately involved, becoming contaminated with volcanic additions and metamorphosed into great complex batholiths. No separate island arcs existed, nor, as James remarks, any separate depositional eugeosynclinal basins. It is true that marginal trenches existed and that some have become filled, but trenches also form along island arcs and may become filled. The deposits of filled trenches which can occur in either case should be distinguished from those constituting the arcs themselves which only occur in the second case.

Where orogeny has produced island arcs the products of volcanism accumulate and become metamorphosed separately from, and without affecting, the sedimentary shelf deposits⁶. It has thus been logical to interpret the extensive and little altered fold belts of the Acadian, Appalachian, Antler, Sonoma and similar orogenies as unaltered shelf and marginal sea deposits and identify the associated, but very different, eugeosynclinal deposits such as those of Central Newfoundland, the Piedmont and the Purcell Mountains as former island arcs¹³. The forces which have folded the shelf deposits and which seem to have pushed the arcs inland still require explanation.

We now consider an oceanic plate which enters and is consumed in a subduction zone over a long period of time. There is a good chance that ultimately it will carry in another continent or a continental fragment. That this will create a series of major disturbances and ultimately close the ocean has been accepted by some as an explanation for the several pulses of orogeny in the Caledonian-Appalachian system¹⁴⁻¹⁷.

These arguments suggest that during the Mesozoic the coastline of western North America between Mexico and the Yukon approximately followed the locus of Jurassic volcanic fields and Cretaceous magmatic activity through the Sierra Nevada, north-western Nevada, the Idaho Batholith and the Coast Ranges of British Columbia¹⁸⁻²¹. Stewart has defined a late Precambrian shore-line in this area²². The two are significantly separated only in Central British Columbia and in Nevada opposite the principal fold belts. It has already been suggested that these areas may be new continental additions whose union with North America caused uplift and folding and introduced faunal invasions²³⁻²⁵.

The Mesozoic shoreline departs significantly from the present coast only in the vicinity of Oregon where an embayment is underlain by thin, possibly oceanic, crust²⁶ and seems to be closed by the Cascade Mountains, marking a subduction zone resembling an island arc^{27,28}.

One possible corollary of the proposed Palaeozoic stand-still of North America may be noted. Krenkel observed that during Tertiary time a series of basins divided by elongated swells developed on the African continent²⁹. If this was due to influences from the deeper mantle upon a stationary plate, then the same explanation could apply to the development during Palaeozoic time of many basins and swells across the United States and Western Canada (for example, the Williston, Peace River and Michigan Basins).

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J. TUZO WILSON
KEVIN BURKE

Erindale College,
University of Toronto

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Periodicity in Whitecaps

WHILE flying low over the ocean between Adelaide and Kangaroo Island (36° S, 137° E) one of us (J. S. T.) observed the following interesting phenomenon.

In a moderate wind of 6-9 m/s from the north, whitecaps were being formed sporadically, each appearing for about a second or less and then disappearing, leaving a streak of foam that would persist for a few seconds. Strikingly, the whitecaps would generally appear in succession, each making its appearance down-wave of the previous one. The interval between the appearance of successive whitecaps was from about 8 to 10 s.

Another of us (M. D.) has made similar observations from the deck of a ship steaming westwards in the N. Atlantic (53° 20' N, 37° 54' W) in a cross-wind of 30 knots and of nearly constant direction of 350°. Again, whitecaps were observed to appear in succession, each down-wave of the last, with a periodicity of about 12.2 s. The period of the larger waves, on the other hand, was substantially less, being about 8.7 s, as determined visually.

We offer the following explanation. Sea waves are not of uniform amplitude, but occur in groups of high and low waves¹. The wave envelope progresses with the group velocity C_g , which for waves in deep water is about half² the phase velocity C . Individual wave crests will appear therefore at the rear of a group, move forward through the group and diminish again towards the front (Fig. 1). The maximum wave amplitude will be attained near the centre of the group, at which point breaking may occur. Now progressive waves tend to break at a certain limiting value of the wave steepness (= wave amplitude/wavelength) for which the downward acceleration of the crest reaches $1/2g$ (see ref. 3). Provided that the wavelength of the dominant waves is nearly constant, this limiting condition will occur at a certain critical value of the wave amplitude, as indicated in Fig. 1. At this critical amplitude a whitecap will appear and will persist until the wave amplitude falls below the critical amplitude.

Because breaking represents the greatest loss of wave energy, which is replenished much more slowly by the wind³, the critical amplitude will be exceeded only slightly. Hence not more than one wave in each group will normally be breaking at any given time. In some groups there may be no breakers.

Thus, breaking will occur periodically, as each crest in succession advances through a group and attains the critical amplitude. But because the wave envelope itself is advancing with the group velocity, each whitecap will occur further forward than the previous one. The periodicity of the whitecaps is determined by the velocity of the wave crests relative to their envelope, which equals $(C - C_g)$. For waves in deep water, this velocity is $C/2$. So we would expect the time interval between successive whitecaps to be just twice the wave period.

This is confirmed by a simple experiment which we have performed in a wave tank of length 45 foot, width 2 foot, and mean depth 2 foot. Waves of period 0.5 s (effectively deep-water waves) were generated at one end of the tank. By

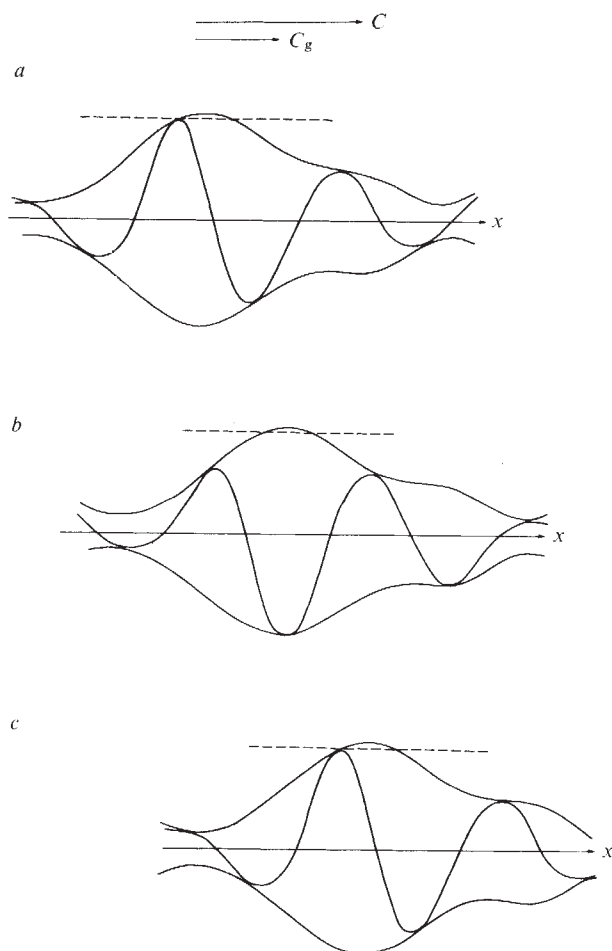


Fig. 1 Schematic drawing of a wave crest advancing through a group of high waves. The broken line indicates the critical wave height for breaking. In *a* and *c* the wave crest is breaking. In *b* there is no breaking. The time between *a* and *c* is two wave periods.

starting the wave-maker from rest, we produced a train of waves progressing down the tank, the front advancing with the group velocity. The form of the wave envelope near the front is described theoretically by a Fresnel integral, which locally exceeds the steady wave amplitude following the front by about 9%. If the steady wave amplitude is slightly less than critical, then on passing through the front of the group each wave crest is made to break. The period of breaking was observed to be double the wave period.

By starting and then stopping the wavemaker gradually we found it possible to generate a finite group of waves which could be made to begin breaking at a certain point along the tank. The period of breaking was then also observed to be twice the wave period.

At the time of the observations near Kangaroo Island, mentioned above, the period of the highest wave, corresponding to a mean wind-speed of 7.5 m/s and a fetch of 100 km, would be expected to be about 4.5 s, or about half the period of the whitecaps. This would be in accordance with our hypothesis.

On the other hand, the shipboard observations suggest that the ratio of whitecapping period to wave period generally lies between 1 and 2. The explanation may be that in a locally generated wave field the frequency spectrum is not always narrow, and that the shorter, steeper waves are helped to break by the orbital motion within the larger waves⁴, which would not break by themselves. The visual observation of wave period is related to surface elevation, whereas for the purpose of wave breaking it is the spectrum of wave steepness which is relevant. The spectrum of surface elevation has a maximum at lower frequency than the spectrum of wave steepness, so that

this would tend to reduce the apparent ratio of whitecap period to wave period. Against this is the tendency of an observer to ignore the breaking of the shorter waves and to record only the larger and more persistent whitecaps.

In the observations just quoted the wind had veered through 180° within the previous 12 hours. There may therefore have been some swell in the opposite direction which would have tended to increase the rate of wave breaking.

Under favourable conditions, that is to say with a steady wind strength and direction, we may expect the period of the whitecaps to be more nearly twice the wave period. If so, then the aerial observation of whitecaps provides a ready means of estimating both the wind-speed and the altitude of an aircraft. For, dividing the period of the whitecaps by two, we obtain the wave period, and hence the phase velocity C , which is related to the wind speed⁵. Knowing the wave period we may also calculate the actual wavelength, and from the apparent wavelength, as seen by an observer in the aircraft, we may calculate the height of the observer. Knowing the wavelength, and that the highest waves are in a breaking condition, we may also estimate the maximum and r.m.s. wave amplitudes^{1,3}.

Two further possible consequences may be noted. (1) Not only breakers but other features of the surface elevation will tend to occur periodically in the ocean. Thus over any moderate area of the sea surface, say a few wavelengths in diameter, the surface elevation may be expected to repeat itself roughly every two wave periods. This phenomenon should be observable, for example, in the intensity of radar backscatter⁶ or of underwater acoustical backscatter from the free surface, for sufficiently narrow beams. (2) A whitecap represents a local disturbance to the normal laminar propagation of surface waves. The disturbance is intermittent, but its propagation speed being one-half the phase-speed of the dominant waves, we may expect a transfer of energy to waves having one-half the dominant wave period, or one-quarter the wavelength. Spectral maxima at twice the dominant wave frequency are indeed observed⁷. There may also be a transfer of energy to much longer waves, namely those with wavelengths comparable to the envelope of the dominant waves. But the length of a wave group not being uniform this reaction will be less specific.

These suggested effects of breaking waves are distinct from the weak non-linear interactions already well known in the literature⁸.

We have discussed waves in deep water only. In shallow water the phase velocity is more nearly equal to the group velocity, and the occurrence of breaking is determined largely by the ratio of the wave height to the local depth of water⁹. Thus the situation is quite different.

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M. DONELAN

Canada Centre for Inland Waters,
Burlington, Ontario

M. S. LONGUET-HIGGINS

National Institute of Oceanography,
Wormley, Godalming, Surrey

J. S. TURNER

Department of Applied Mathematics
and Theoretical Physics,
Silver Street, Cambridge

Received March 11, 1972.

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Lead Corrosion and Oil Oxidation

THE reaction between metallic lead and hydrocarbon peroxides has been extensively studied¹⁻⁵ but always from the standpoint of what happens to the lead. Little attention has been given to the way this reaction can alter the subsequent oxidation reactions of the hydrocarbons. We show that when lead is corroded by the oxidizing hydrocarbon, the oxidation chain reaction is stopped as effectively as if a classical oxidation inhibitor was present. In this respect, lead seems to be unique among metals. Other metals such as copper, iron or aluminium tend to catalyse the oxidation reaction; lead inhibits it.

This reaction is not interesting only because of its unique character chemically, but also because it has a very practical application in the behaviour of motor oils. Copper-lead bearings are used in many engines, and their corrosion can be a limiting factor if the motor oil is not properly inhibited. Several engine test procedures have been set up to measure this property (for example, CRC L-4 and L-38, Petter W-1). Again, there has been little interest in what happens to the oil in these tests, but only in what happens to the lead.

The reaction is complicated by the fact that lead surfaces tend to be rapidly passivated. Static oxidation tests using strips of lead or pieces of copper-lead bearings do not reproduce the rubbing conditions of the engine. As a result none of these tests has gained any wide acceptance as a screening test for the engine.

Wilson⁵ partially solved this problem by brushing the lead strips at intervals of 15 min in order to remove the passivated layer. We now report a further improvement, which consists of running the test under conditions of cavitation erosion. This erosion action provides a metal surface that is continuously clean.

Table 1 Oxidation Results in Cavitation Conditions (Iron Tips)

Ball	Oxidized products, %	Acids, %	Colour
Fe	3.2	0.47	Dark brown
Al	2.7	0.34	Brown
Cu	2.4	0.34	Brown
Pb	2.2	0.20	Yellow
Fe*	1.5*	0.30*	Dark brown*

* No cavitation.

The apparatus is the same as that used by Tao⁶ to study cavitation in liquids. A 20 kHz ultrasonic horn is vibrated close to a metal ball immersed in an oil. In our tests the horn was AISI 304, the balls were variously high purity Al, Cu, SAE 52100 steel, and Pb. The oil was a refined paraffinic mineral oil into which air was bubbled at a rate of 2.7 l/h; the temperature was 140° C. After 7 h of reaction the oil was analysed for total oxidized products using a Florisil column, and for organic acids using an ion-exchange resin. In the presence of lead, oil degradation is much less than for the other metals (Table 1). This is especially obvious in the visual appearance, lead giving a transparent yellow oil, whereas with the other metals (or even in the absence of metals) the oil is an opaque brown. Similar results were obtained when the horn was made from other metals, for example, aluminium.

In a parallel set of experiments, the oil was analysed for peroxide content and soluble metals. As expected, the oil oxidized over lead showed only half the peroxide content and contained appreciable amounts of soluble lead in the form of basic lead soaps.

These results are exactly matched by actual engine runs, using the old CRC L-4 procedure which requires inspection of the engine for sludge and carbon deposits and inspection of the oil for oil-insoluble matter (Table 2). Two separate tests were run on each oil, first with the standard Cu-Pb bearings and then with non-corroding Babbitt bearings. With Cu-Pb bearings, the weight loss due to corrosion was often very high, but the engine was relatively clean and the oil was not seriously degraded. With Babbitt bearings there was no corrosion of bearing metal, but engine deposits were heavy and the oil had a large amount of oxidized products.

Table 2 CRC L-4 Engine Test Data

Oil	Bearing metal	Engine sludge demerit*	Used oil acidity mg KOH/g	Bearing weight loss, g
SAE 10	Cu-Pb	0.1	8.5	8.91
SAE 10	Babbitt	3.3	15.2	0.0
SAE 20	Cu-Pb	0.4	3.6	3.10
SAE 20	Babbitt	4.3	7.4	0.0

* 0 = clean; 10 = maximum deposits.

Other deposits in the engine, such as ring-zone carbon and piston-skirt lacquer, paralleled the sludge deposits: other used-oil properties, such as viscosity increase and insoluble matter, paralleled the acidity.

It is obvious that the lead, in being corroded, has altered the course of the oxidation reactions. Probably the peroxides, which are the chain carriers of reaction, have reacted with the lead and have thus destroyed themselves. The lead has thus acted as an oxidation inhibitor in the classical sense.

J. K. APPELDOORN
P. PACOR
V. RIDDEI

*Esso Italiana,
Centro Ricerche,
Rome*

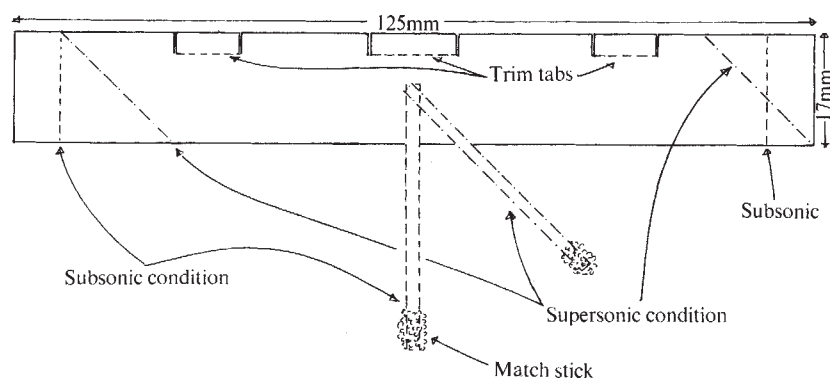
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Stability of Swivel Wing Supersonic Aircraft

THE model^{1,2} presented by R. T. Jones for a swivel wing supersonic transport does not reveal the inherent stability of trim when the wing is rotated. This stability may very simply be demonstrated by a paper model (Fig. 1). A straight strip of paper is taken and trim tabs are cut as shown. For the subsonic flight condition, wing tip fins are bent up at right angles to the span and the nose weight, a wooden match split halfway along its length, is set parallel to the fins. To convert to the supersonic swivel winged condition, new fins are bent at 45° to the span and the match is moved to a new position parallel

Fig. 1 Design for a model swivel wing aircraft made from paper. The paper is about 0.1 mm thick and fins are bent up for the cross winged subsonic condition as shown (---). For the diagonal winged supersonic condition, new fins are bent at 45° to the span (—) and the match stick nose weight is moved from position --- to position —.



to the fins. Straight flight is achieved by bending the fin tips and trim for level flight by bending up the trim tabs and adjusting the extension of the match stick.

When stable flight in the cross wing condition has been achieved, conversion to the diagonal condition is made by bending up the 45° fins and swivelling the match stick. Other trim changes are not required for stable flight.

H. C. BENNET-CLARK

Department of Zoology,
University of Edinburgh,
West Mains Road,
Edinburgh EH9 3JT

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¹ *Nature*, **237**, 252 (1972).

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BIOLOGICAL SCIENCES

Free Tryptophan in Plasma and Brain Tryptophan Metabolism

SYNTHESIS in the brain of the putative transmitter 5-hydroxytryptamine (5HT) is influenced by the concentration of tryptophan; the rate limiting step in 5HT synthesis is the hydroxylation of tryptophan¹ by tryptophan hydroxylase, which is normally unsaturated with substrate². Often when 5HT turnover in the rat brain increases—as indicated by higher concentrations of its metabolite 5-hydroxyindoleacetic acid (5HIAA)—there is a corresponding increase in brain tryptophan. This occurs when the rats are given various drugs known to increase brain 5HT synthesis³, deprived of food^{4,5}, or immobilized⁵. Therefore the control of brain tryptophan concentration is clearly of some importance.

Although brain tryptophan is presumably derived from plasma, the brain and plasma concentrations of this amino-acid are not significantly correlated. Plasma tryptophan con-

centration is unaltered when brain tryptophan increases upon food deprivation or immobilization^{4,5}. However, only a small fraction of plasma tryptophan, as conventionally determined, is free and thus directly available to the brain; most plasma tryptophan is bound to plasma albumen sites⁶.

We have found that increases of brain tryptophan upon food deprivation or immobilization stress are associated with increases of free tryptophan in plasma, and that the proportion of total plasma tryptophan in the free (ultrafilterable) state increases in response to these and other stimuli.

We used male Sprague-Dawley rats, 180–240 g (Carworth-Europe, Alconbury), kept at a constant temperature in acoustically lagged housing with a 0600–1800 h light-dark cycle. Rats were immobilized for 3 h⁷, or deprived of food but not water for 24 h (Tables 1 and 2). The animals and controls were then killed by guillotine at 1600–1700 h; their brains were removed, frozen with solid CO₂ and stored at –20° C until needed to determine tryptophan and (in the food deprivation experiments only) 5HT and 5HIAA as previously described⁵. Blood was collected into heparinized tubes, immediately centrifuged and the plasma frozen with CO₂ and stored until tryptophan determination by the method of Denkla and Dewey⁸. Tryptophan was determined on whole plasma and also on 0.4–0.5 ml. of plasma ultrafiltrate prepared by centrifuging 1.0 ml. of plasma in a 'CF 50 Diaflo' membrane cone (Amicon) at 800g for 30 min at room temperature. Unesterified fatty acids in the plasma were determined by the method of Laurell and Tibbling⁹.

The effects of 24 h food deprivation are shown in Table 1. In agreement with previous results⁵, brain 5HIAA and tryptophan concentrations both rose significantly. Though total plasma tryptophan did not change significantly there was a large and significant increase of free tryptophan associated with a comparable percentage increase of fatty acid¹⁰.

The effects on the disposition of tryptophan when the rats were immobilized for 3 h are shown in Table 2. Changes of free tryptophan in plasma and brain tryptophan are qualitatively similar to those on food deprivation but quantitatively less. The mean brain tryptophan change was quantitatively similar to that found previously⁵ but, unlike the previous result, was not statistically significant.

Table 1 Effect of 24 h Food Deprivation on Plasma Total and Free Tryptophan, Plasma Unesterified Fatty Acids and Brain Tryptophan, 5-Hydroxytryptamine (5HT) and 5-Hydroxyindoleacetic Acid (5HIAA) in Rat

	Plasma		Unesterified fatty acids (m.eq./l.)	Tryptophan	Brain (µg/g wet weight)	
	Total (µg/ml.)	Free (µg/ml.)			5HT	5HIAA
Controls	21.15 ± 2.75 (20)	1.23 ± 0.35 (20)	0.34 ± 0.10 (8)	2.26 ± 0.41 (20)	0.53 ± 0.06 (12)	0.42 ± 0.04 (12)
Deprived of food but not water for 24 h	20.57 ± 4.98 (20)	3.08 ± 1.17 (20)	0.88 ± 0.17 (8)	3.94 ± 0.73 (20)	0.54 ± 0.05 (12)	0.62 ± 0.13 (12)
% change	–3	+150	+159	+74	+2	+48
P	N.s.	<0.001	<0.001	<0.001	N.s.	<0.001

Control animals were caged in groups of four–eight and allowed food and water *ad libitum*. Results are given as means ± one standard deviation. Numbers of animals are shown in parentheses.

Table 2 Effect of 3 h Immobilization on Plasma Total and Free Tryptophan and Brain Tryptophan in Rat

	Plasma tryptophan ($\mu\text{g/ml.}$)		Brain tryptophan ($\mu\text{g/g wet weight}$)
	Total	Free	
Control (8)	23.33 $\pm$ 2.93	2.44 $\pm$ 0.85	3.19 $\pm$ 0.68
Immobilized for 3 h (8)	19.83 $\pm$ 3.29	3.58 $\pm$ 1.12	3.76 $\pm$ 0.99
% change	-15	+47	+18
P	N.s.	<0.05	N.s.

Animals were caged in groups of four and allowed food and water *ad libitum* before immobilization, during which both control and test animals were singly housed without food or water. Results given as means $\pm$ one standard deviation. Numbers of animals are shown in parentheses.

Presumably as a consequence of these increases of free tryptophan in plasma, the brain tryptophan concentration rises and leads to the increased 5HT turnover indicated by increased amounts of brain 5HIAA. The concentration of 5HT changes little, either because our procedures cause stress that increases the firing of 5HT neurones and hence 5HT destruction, or 5HT stores are replete and additional 5HT synthesized is converted by monoamine oxidase to 5HIAA. The changes in brain tryptophan concentration could well play an important central regulatory role in some circumstances, ensuring that 5HT stores are replete. Another possibility of regulation is suggested by the recent finding¹¹ that peripheral tryptophan administration decreases the firing of central 5HT neurones.

Association of the changes in plasma tryptophan with increased unesterified fatty acid concentrations suggests that they could be mediated by increased sympathetic nervous activity since plasma fatty acid concentrations increase during noradrenaline infusion¹² and stresses in which plasma catecholamine increases¹³; the plasma tryptophan would decrease on ganglionic blockade¹². The relationship between unesterified fatty acid and free tryptophan concentrations in plasma was investigated further by exploiting the release into plasma of tissue lipases after injection of heparin, which increases plasma fatty acid concentration¹⁴ (Table 3). The percentage increases of both unesterified fatty acid and free tryptophan concentrations were comparable after heparin injection. Whether the fatty acid changes cause the tryptophan changes or are merely associated with them is unclear; there is evidence that heparin may increase sympathetic activity¹⁵.

Table 3 Effect of Heparin on Plasma Total and Free Tryptophan and Unesterified Fatty Acids in Rat

	Tryptophan		Unesterified fatty acids (m.eq./l.)
	Total ($\mu\text{g/ml.}$)	Free ($\mu\text{g/ml.}$)	
Controls (5)	18.61 $\pm$ 2.52	2.16 $\pm$ 0.29	0.25 $\pm$ 0.04
Heparin 5,000 IU/kg (5)	15.20 $\pm$ 3.84	4.08 $\pm$ 0.94	0.47 $\pm$ 0.11
% change	-18	+89	+88
P	N.s.	<0.01	<0.01

Injections were made by tail vein using 1.0 ml/kg, and control animals were injected with saline. Blood was collected 15 min after the injections. Results given as means $\pm$ one s.d. Numbers of animals are shown in parentheses.

We conclude that changes of free tryptophan concentration in plasma may influence brain tryptophan and hence brain 5HT turnover in many circumstances. For example, in depressive illness there is evidence for defective brain 5HT metabolism¹⁶ and for low tryptophan concentration in the cerebrospinal fluid¹⁷. Since free tryptophan but not albumen-bound tryptophan in plasma can gain access to the cerebro-

spinal fluid, free tryptophan concentration in plasma may be low in depression and lead to low brain 5HT synthesis.

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P. J. KNOTT
G. CURZON

Department of Neurochemistry,
Institute of Neurology,
Queen Square,
London WC1

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Extraordinary Incidence of Electrophoretically Silent Genetic Polymorphisms

ELECTROPHORETICALLY detected genetic polymorphisms are ubiquitous. In man, for example, they emerged by chance in two^{1,2} out of four¹⁻⁴ kinds of protein examined in this laboratory during 1960-1962. Since then, polymorphisms have been documented for about 33% of those human loci⁵ and as many as 80% of those *Drosophila* loci⁶ whose products can be analysed electrophoretically. Despite this abundance, the variation represented by electrophoretically detected polymorphism is comparatively restricted. In general, no more than a few kinds of frequent alleles can be detected at any one locus. At three haemoglobin loci (α , β , and δ), for example, a total of only five kinds of variant alleles with frequencies >0.03 were found in a survey of 404 individuals representing nine primate genera⁷. Thus, among $3 \times 2 \times 440 = 2,640$ allele products, the incidence of different kinds of common variant alleles was 5/2,640.

In this report attention is called to an incidence of electrophoretically undetectable polymorphisms which, at the same loci and in the same general populations, is much greater than just described. Among approximately 70 haemoglobin allele products examined, at least 5 out of 7 kinds of electrically neutral variants are determined by reasonably common alleles. These electrophoretically silent polymorphisms resemble, with respect to their sequence positions and kinds of substitution involved, the fixed amino-acid changes accumulated during evolution of primate haemoglobins⁸. If⁹⁻¹² fixed amino-acid substitutions are chiefly the result of non-adaptive mechanisms then it is possible that electrically neutral polymorphisms may represent the transient phase¹³ of non-adaptive molecular

Table 1 Electrophoretically Neutral Variants among Primate Haemoglobins

Genus	Chain position	Variant residue	Common residue	Variant allele frequency
<i>Ateles</i>	δ^9	Ala	Ser	2/12
<i>Saimiri</i>	β^{76}	Ala	Thr	2/18
<i>Gorilla</i>	δ^{126}	Ala	Val	4/12
<i>Gorilla</i>	δ^{113}	Leu	Val	2/12
<i>Cebus</i>	β^{13}	Ala	Thr	2/16
<i>Macaca irus</i>	α^8	Ser	Thr	3/11
<i>Gorilla</i>	$3\alpha^{131 \text{ or } 133}$	Asn	Ser	1/2

Analyses supporting entries in first four and sixth rows are summarized elsewhere^{7,8}. Data supporting last row entry are unpublished. Quality of analyses is illustrated by amino-acid assay of tryptic peptide β -2 (residues 9-17) which in a *Cebus* position β^{13} heterozygote contained 1.02 Lys, 1.44 Thr, 1.01 Ser, 0.98 Gly, 1.4 Ala, 0.99 Val, 1.12 Leu, and 1 Trp residues, whereas in a homozygous wild type *Cebus* contained 0.99 Lys, 1.93 Thr, 1.03 Ser, 0.96 Gly, 1.08 Ala, 0.95 Val, 1.05 Leu and 1 Trp residues. In most cases, determinations of variant allele frequency are based on analyses of single peptides from individual animals. In terms of estimating incidence of kinds of variants (see text), analyses of single peptides do not contribute materially to the total of ~70 allele products each of whose sequence was more or less fully determined.

evolution. The electrophoretically silent polymorphisms are, in any case, better candidates for this role than the electrophoretically detected polymorphisms whose existence now seems to depend on balanced heterozygote advantage⁶.

In an earlier study⁷, three kinds of heterozygotes with electrically neutral mutations were detected by chance during sequence analysis of primate haemoglobin chains representing about 38 allele products. All variants, together with updated allele frequencies, are shown as the first three entries in Table 1. It was unclear⁷ whether any or all of these three were common, and thereby part of a genetic polymorphism, or rare. Interpretations of polymorphic status were hampered by possible relatedness among carrier individuals. Subsequently it became evident that the *Ateles* δ^9 and *Gorilla* δ^{126} variants (Table 1) each occur in several unrelated animals⁸ and are therefore presumably frequent. *Bona fide* polymorphism at *Saimiri* β^{76} remains uncertain since it is possible that both variant individuals derive from the same wild type. A further common variant was also found, a gorilla homozygous for δ^{113} Leu and thereby unlike δ^{113} Val wild type. Although no other δ^{113} Leu variants were encountered, it seems unlikely that a homozygote would have been ascertained through peptide analysis unless the variant had reached reasonably high frequency. Finally, an electrophoretically silent variant of indeterminate frequency (last named, Table 1) was detected during sequence analysis of the unusual 3α product in *Gorilla*¹⁴.

These electrophoretically neutral variants occurred in an instructive pattern. The first variant was found among the first 10 primate allele products examined, the second variant appeared in the next 10 products, and so on, making a tally of 5 in 52. It appears that additional kinds of silent variants, often common in frequency can be expected among approximately every 10 haemoglobin allele products examined. Reports by others⁸ support this. During analyses of about 20 primate haemoglobin allele products, a β^{13} Ala/ β^{13} Thr heterozygote was found in *Cebus albifrons*¹⁵ and an α^8 Ser/ α^8 Thr heterozygote in *Macaca irus*¹⁶. I then examined appropriate peptides from other unrelated members of these species and established that each variant was comparatively common (Table 1). The overall incidence of different kinds of seemingly frequent alleles among electrophoretically silent variants thereby becomes 5 in about 70 allele products, and may be greater. It is tempting to regard all of the 7 variants listed (Table 1) as comparatively common. Each was ascertained after examining only one or two individuals from each genus. The chance detection of a rare variant in this way seems small indeed.

The electrically neutral variants are remarkable not only for their high incidence but also for their location. Five of the

seven mutants (Table 1) occupy positions (α^8 , β^{13} , β^{76} , δ^9 and δ^{126}) which are part of the comparatively small number of sites where fixed mutations have accumulated during primate haemoglobin evolution^{7,8}. Three variants (*Saimiri* β^{76} Ala, *Cebus* β^{13} Ala, and *Macaca irus* α^8 Ser) may be remnants of transient polymorphisms now largely gone to fixation. *Saimiri* is the only New World species aside from *Cebus* to which it may be closely related, to possess β^{76} Thr as putative wild type; other New World species⁸ have β^{76} Ala. The *Saimiri* β^{76} Ala variant is thus either the result of back mutation or a relict of transient polymorphism. The same choice applies to the β^{13} Ala variant in *Cebus*. Apart from *Saguinus* this genus is the only one from the New World with β^{13} Thr wild type; others⁸ have β^{13} Ala. In the case of *M. irus* the predominant form, α^8 Thr, resembles the presumptive Old World stem line⁸ but differs from the α^8 wild type of *M. mulatta*¹⁶. The α^8 Ser variant of *M. irus* may thus be a relict from the time of taxonomic divergence from *M. mulatta*.

The electrically silent variants in Table 1 resemble the fixed amino-acid substitutions accumulated during evolution in yet another way. As in polymorphisms, recognizable changes fixed during primate haemoglobin evolution exhibit a relative deficiency of electrophoretically detectable mutations. Among 48 recognizable mutations fixed in α and β chains since divergence of Old and New World lines of primate descent, only 4 involve electrophoretic alterations from ancestral residues whereas 19 such changes are expected from random permutation of codons at the positions involved⁸.

The similarities between the variants shown in Table 1 and the changes accumulated during primate evolution suggest that the electrically neutral variants may represent the transient phase¹³ of protein evolution. If this aspect of evolution is chiefly non-adaptive in origin then the extraordinary incidence in kinds of electrophoretically undetectable variants provides abundant substrate for a process from which very few mutations survive to fixation. Such speculation may, of course, be premature since the ultimate significance of the findings will depend on whether the same is true for other taxa and other proteins.

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SAMUEL H. BOYER

Division of Medical Genetics and the Clayton Laboratories,
Department of Medicine,
Johns Hopkins University School of Medicine and Hospital,
Baltimore, Maryland 21205

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Milk Proteins

I WISH to draw attention to some errors in the recent article by Winterburn and Phelps¹ on the significance of glycosylated proteins.

First, in the diagram of protein composition of cow's milk they include both α -casein and κ -casein. κ -Casein is in fact a component of the fraction originally known as α -casein. The α -casein can be separated into κ -casein and α_s -casein^{2,3}. If Winterburn and Phelps have confused α -casein and α_s -casein, then " α -casein" should be replaced by " α_s -casein". It should not be classified as a glycoprotein as there are no reports that α_s -casein contains significant amounts of carbohydrates. On the other hand, if α -casein is used in the generally accepted sense, then it should be recognized that it contains carbohydrates only because of the presence of κ -casein. In this case κ -casein should not be allocated an additional space as this would mean that it was being included twice. In the light of current knowledge it is usual to consider κ -casein and α_s -casein as separate identities.

Furthermore, a proportion only of κ -casein is glycosylated^{4,5}, yet it is classified as a glycoprotein. α -Lactalbumin is similar in this respect as it contains glycosylated components. Therefore it is difficult to understand why this protein is not included in the same category as κ -casein.

It is also stated in the article that the liberation of a glycopeptide containing the majority of the carbohydrate destroys the micelle-stabilizing properties of the protein. This is true, but to put the matter in perspective it should also be pointed out that almost the same effect is achieved in molecules of κ -casein which are devoid of carbohydrates. Hence the implication that the carbohydrates have an essential role in the micelle-stabilizing properties of κ -casein is not valid. In fact, it is widely agreed that the κ -casein carbohydrates are not essential for either rennin action⁶ or for the micelle-stabilizing power itself⁴.

It may be that recognition of these errors does not greatly affect the conclusions of the authors, but nevertheless it is desirable that conclusions are based on a sound assessment of the facts.

J. V. WHEELLOCK

School of Biological Sciences,
The University,
Bradford,
Yorkshire BD7 1DP

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Uptake of Bacteria by Isolated Higher Plant Protoplasts

ISOLATED higher plant protoplasts can take up particles, by endocytosis, into vesicles in their cytoplasm. The uptake of tobacco mosaic virus, ferritin, and polystyrene latex spheres has been reported¹⁻⁴. Particles up to 0.3 μ m have been observed to be actively taken up¹. The size of some vesicles as seen in thin sections of fixed and embedded isolated protoplasts, however, indicates that particles considerably

larger than 0.3 μ m could be accommodated in such vesicles in the cytoplasm. We have therefore investigated the possibility of uptake of bacteria into higher plant protoplasts, using *Rhizobium* and pea leaf protoplasts as our experimental system.

Fully expanded leaflets were excised from 5-7 week old pea plants (*Pisum sativum* var. Little Marvel), wetted by immersion in 1% sterile 'Teepol' (BDH Chemicals Ltd.) (3 min), and surface sterilized in 1.5% sodium hypochlorite solution (10 min). The sterilant was removed by 8 successive washes with sterile water. 1.5 g fresh weight of peeled leaflets² was incubated with 5 ml. of a mixture of 5% Meicelase (Meiji Seika Kaisha Ltd, Tokyo) with 5% Macerozyme (All Japan Biochemicals Co. Ltd., Nishinomiya) in 25% sucrose (pH 5.8) containing 200×10^6 *Rhizobium leguminosarum* (Rothamsted Culture Collection Cat. No. 1007). To prepare this mixture, bacteria were cultured in 100 ml. of liquid mannitol yeast-water medium³ in 250 ml. Erlenmeyer flasks incubated at 25° C in the dark on a horizontal shaker (100 r.p.m.), and harvested by centrifugation 3 days after inoculation. Pelleted bacteria were re-suspended in water, an aliquot removed for cell density determination, and the volume adjusted so that 1 ml. of this suspension, when added to 4 ml. of the filter sterilized enzymes in 31.25% sucrose, gave the required bacterium density and a final plasmolytic concentration of 25% sucrose. Leaf pieces were incubated for 20 h in this enzyme-bacterium mixture in the dark at 25° C. The released protoplasts were freed of leaf debris by straining through a fine wire gauze, and the protoplast-enzyme-bacterium suspension centrifuged at 225g for 5 min. The surface film of intact protoplasts was washed to remove enzymes and bacteria by re-suspending six times in 25% sucrose, then centrifuged at 225g for 5 min. Protoplasts were fixed for electron microscopy for 16 h at 22° C, in 3% glutaraldehyde in 0.1 M sodium phosphate buffer (pH 7.1) containing 25% sucrose. They were washed in 0.1 M sodium phosphate buffer, and post-fixed for 3 h at 2° C in 1% osmium tetroxide buffered with 0.1 M sodium phosphate (pH 7.1). Thin sections were prepared as previously described⁶.

Observations of ultrastructure showed the presence of *Rhizobium* localized within membrane bounded vesicles in the cytoplasm of the isolated pea leaf protoplasts (Fig. 1a). There were also bacteria in vesicles deep within the cytoplasm of many protoplasts and within transvacuolar cytoplasmic strands (Fig. 1b and c). Some burst protoplasts with bacteria still enclosed within vesicles were also present in the cytoplasmic debris. Morphologically the bacteria appeared unaffected by the enzyme mixture used to degrade the leaf cell walls, or the plasmolyticum, or by being enclosed within vesicles in the host cell cytoplasm. Constricted bacteria were present in some vesicles (Fig. 1a), but it is not clear whether they were dividing within the vesicles following uptake, or whether they had commenced division prior to uptake.

Pea leaf protoplasts take up bacteria only during enzymatic digestion of the surrounding cell wall. Uptake did not occur when peeled leaf pieces were incubated with the exposed mesophyll cells in contact with bacteria, nor when isolated protoplasts were incubated with *Rhizobium*. The uptake is thought to occur by engulfment of bacteria into vesicles formed by invagination of the plasmalemma during plasmolysis and concomitant degradation of the cell wall, rather than by a strictly endocytotic process. Since the membranes of the vesicles containing the bacteria are derived from the plasmalemma, they have an origin directly analogous to that of membranes enclosing bacteria which enter root nodule cells via the normal infection thread⁷. Approximately 5% of the protoplasts examined contained up to five bacteria per protoplast section, with two or three bacteria within some vesicles.

It seems likely that this plasmolytic uptake of *Rhizobium* by pea protoplasts during their isolation will occur in many microorganisms and protoplast systems. Such investigations may produce new endosymbiotic relationships. The symbiotic

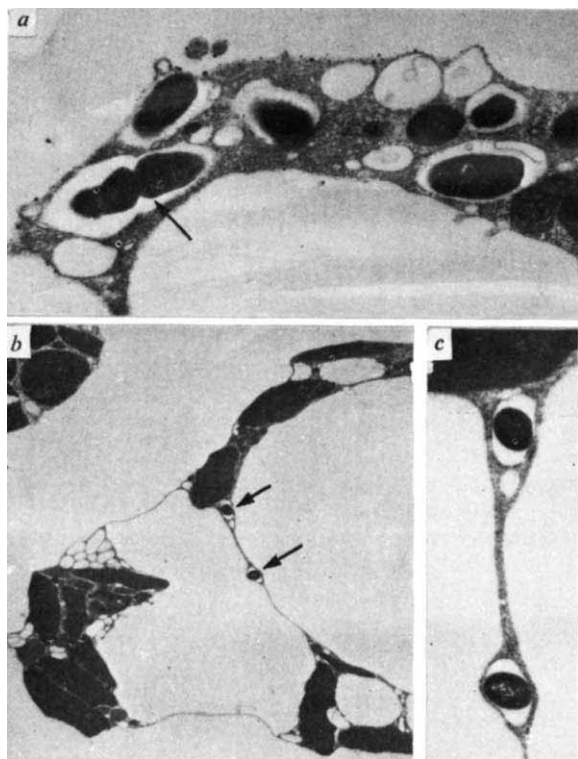


Fig. 1 *a*, Bacteria and a constricted bacterium (arrowed) taken up into vesicles in the cytoplasm of an isolated pea leaf protoplast. ($\times 10,000$.) *b*, Bacteria (arrowed) in a transvacuolar cytoplasmic strand. ($\times 3,000$.) *c*, The cytoplasmic strand of *b* showing bacteria within membrane-bounded vesicles. ($\times 10,000$.)

nitrogen fixing relationship of *Rhizobium* with soybean cells *in vitro* has recently been established⁸, but the extent to which nitrogen fixation in such legumes either *in vitro* or *in vivo* is genetically controlled is still unknown⁹. We are currently investigating whether non-legumes will take up bacteria such as *Rhizobium* and *Azotobacter* by this plasmolytic method, and whether there is any difference between legumes and non-legumes in the behaviour of these bacteria within the uptake vesicles. It has been postulated that during evolution, part of the bacterial DNA concerned with the nitrogenase system was transferred to the legume and became part of its DNA complement, and as a direct result of this transfer the *Rhizobium* and the legume became dependent on their symbiosis for the production of nitrogenase, the enzyme essential for nitrogen fixation⁹. Any uptake of a free nitrogen fixing bacterium such as *Azotobacter* into vesicles of non-legume protoplasts, with the possibility of intracellular degradation of the bacterium and transfer of released DNA, should provide a new experimental system to test this hypothesis.

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M. R. DAVEY
E. C. COCKING

Department of Botany,
University of Nottingham

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Hypersensitivity as a Consequence, Not the Cause, of Plant Resistance to Infection

THE early observations of Ward¹ and Stakman² on wheat rusts (*Puccinia recondita* Rob. ex Desm. and *P. graminis* Pers.) resulted in the formation of the concept of the hypersensitive response of a resistant host plant to infection. This type of resistance is characterized by disorganization, browning and death (necrosis) of cells at the infection sites^{3,4}.

It has been suggested that the growth of pathogens is restricted in cells which react in a hypersensitive manner, and the infecting agents are damaged or even killed in necrotic tissues. This phenomenon was originally recognized in incompatible host-pathogen interactions, mostly with obligate parasites. The classical studies of Müller and Börger⁵ on "phytoalexins" were believed to be important for understanding the role of the hypersensitive death of host cells in plant resistance to infection. Phytoalexins can be defined as substances produced by plants after infection and adverse treatments, and responsible for resistance to infecting agents. Large amounts of phytoalexins are produced in hypersensitive (resistant) plants.

We would like to report here on experiments which show that hypersensitive necrosis connected with the production of a phytoalexin is only a consequence, not the cause, of resistance of potato, beans and wheat to different fungi (*Phytophthora infestans* and rusts). We were able to induce a hypersensitive type of necrosis in potato tuber-, wheat leaf-, and in bean leaf-tissues infected with compatible races of pathogens, when these pathogens were inhibited or prevented from further growth in the host tissues.

Table 1 Growth of *Phytophthora infestans*, the Degree of Tissue Necrosis and Amount of Rishitin in Gülbaba (*r*) Potato Slices Treated with Chloramphenicol

Concentration of chloramphenicol (p.p.m.)*	Growth of surface mycelium (%)	Degree of necrosis	Rishitin content ($\mu\text{g g}^{-1}$ fresh weight)†
0 (control)	100	0	0
50	80	+	41
100	40	++	62
200	20	+++	91
400	0	++++	100
800	0	++++	83

* Chloramphenicol was applied 1 h before inoculation.

† Extraction of rishitin was carried out by the procedure of Sato and Tomiyama¹⁸ 65 h after inoculation from 1.0–1.5 mm thick tuber slices. Rishitin was separated on plates coated with Silica Gel-G using ethyl-ether as a solvent. O.D. of spots at 500 nm was determined in a Unicam SP 800 spectrophotometer.

In the initial experiments *Phytophthora infestans* (Mont.) de Bary race 1.2.3.4 in a compatible potato (cultivar Gülbaba) tuber tissue with *r* gene was inhibited by different concentrations of chloramphenicol applied before or 1–2 h after inoculation (Table 1). Unexpectedly, the compatible host reacted in an incompatible manner forming typical tuber necroses and producing the phytoalexin rishitin (hypersensitive response). Activities of peroxidase and polyphenoloxidase enzymes were also augmented, which is an additional characteristic of the hypersensitive response. Neither the fungus infection nor the treatment with chloramphenicol alone induced necrosis with increased phytoalexin production and with augmented enzyme activities. Only when the fungal pathogen was inhibited

in the host tissues were the symptoms of an incompatible reaction expressed, for example the surface mycelium of the fungus did not appear, tissue necrosis developed and a phytoalexin was produced in the tuber. Chloramphenicol was found by us to be a powerful inhibitor of the growth of *Phytophthora infestans* in different media even in a concentration of 25 p.p.m.⁶. Germination of zoospores, however, was not inhibited, only the mycelial growth. Streptomycin, another inhibitor of *Phytophthora infestans*, in a concentration of 20 p.p.m. exerted a similar action⁷⁻⁹. It induced necrosis in the host-pathogen combination, which was originally compatible.

On the basis of this finding one can infer that the inhibition of growth and death of the pathogen in the host tissue is a requirement in the induction of a hypersensitive type necrosis in the host under natural conditions too. In other words, in a natural host-pathogen interaction which results in incompatibility, it is not the necrosis of host tissues which inhibits or prevents the pathogen from further growth, but before necrosis, unknown mechanism(s) inhibits or even kills the pathogen. The hypersensitive necrosis is the result of the death or inhibition of the growth of the pathogen.

This working hypothesis was supported by previous findings¹⁰⁻¹², according to which homogenates or sonicates prepared from the mycelium of both compatible and incompatible races of *Phytophthora infestans* possess factor(s) which can induce tuber necrosis. We were also successful in producing necrosis inducing factor(s) by sonication even from a compatible race of the late blight fungus (Table 2).

Table 2 Hypersensitive Reactions in Different Host-Pathogen and Host-Toxin Interactions

Host-Pathogen complex	Necrosis	Phytoalexin rishitin
<i>Phytophthora infestans</i>		
Compatible*	0	0
Varietal incompatible†	+	+
Compatible + treatment with chloramphenicol or streptomycin	+	+
Sonicated‡		
Compatible	+	+
Incompatible	+	+
Chloroform-treated		
Compatible§	+	+
Incompatible	+	+
<i>Puccinia graminis</i>		
Compatible¶	0	
Varietal incompatible**	+	
Compatible + treatment with blasticidin-S	+	
+ treatment with Ni(NO ₃) ₂	+	
+ heat treatment	+	
<i>Uromyces phaseoli</i>		
Compatible††	0	
Varietal incompatible‡‡	+	
Compatible + treatment with Ni(NO ₃) ₂	+	
+ heat treatment	+	

* Race 1 or 1.2.3.4 on potato variety Gülbaba (r).

† Race 1 on potato variety Rotkelchen (R₁R₃).

‡ Homogenates of 1 g mycelium 10 ml.⁻¹ distilled water of race 1.2.3.4 or 1 was sonicated with a refrigerated MSE sonifier for 10 min at maximum intensity.

§ Cell-free liquid released by race 1.2.3.4 as a result of treatment with chloroform was applied to tuber slices of potato variety Gülbaba.

|| Cell-free liquid released by race 1 was applied to tuber slices of Rotkelchen.

¶ Race 40 on wheat variety Reliance.

** Race 11 on wheat variety Vernal.

†† Bean variety Pinto infected by *Uromyces phaseoli*.

‡‡ Bean variety Fürj infected by *Uromyces phaseoli*.

The question whether a compatible race of *Phytophthora infestans* is indeed able to release the inducer (endotoxin) of tissue necrosis and phytoalexin accumulation was approached by another experiment. Mycelium of *Phytophthora infestans*

race 1.2.3.4 was placed on a watchglass and held for 24 h in a chloroform atmosphere under a glass jar. The mycelium was killed and the cells released liquid on the watchglass. The liquid was collected and applied to the surface of tuber slices of a compatible potato variety (Gülbaba). As a result, typical tissue necrosis and accumulation of rishitin (a phytoalexin) were demonstrated. Similar results were achieved with an incompatible host-parasite interaction (Table 2). We conclude that endotoxin(s) exists also in the compatible races of the fungus and this is released only after the inhibition or death of the fungal mycelium. In compatible tuber tissues the pathogen supposedly remains undamaged under natural conditions, and, therefore, endotoxin(s) will not be released. Consequently, tissue necrosis or the "hypersensitive" reaction does not occur. In the naturally incompatible potato varieties, however, the pathogen is inhibited or killed by an unknown mechanism of resistance which initiates a series of events: endotoxin(s) will be released, necrosis of host tissues and phytoalexin production will be induced, and, finally, the hypersensitive host response develops.

From these results with the late blight disease, we tried to extend our concept on hypersensitive necrosis as a consequence of resistance to other host-pathogen combinations. *Puccinia graminis* (stem rust) race 40 in leaves of seedlings of a compatible wheat variety (Reliance) was inhibited by brushing the leaves with a 50 p.p.m. solution of blasticidin-S on three subsequent days or with a 0.05% solution of Ni(NO₃)₂ in the chlorotic stage of the disease (Table 2). In this host-pathogen combination, which was originally a compatible one, tissue necroses were visible at the infection sites as a result of the inhibition or death of the established pathogen by chemotherapeutants. It remains to be seen whether or not an inducer (endotoxin) of necrosis similar to those demonstrated in experiments with *Phytophthora* is also produced in this disease.

In another host-pathogen combination, bean rust (Pinto bean—*Uromyces phaseoli* [Pers.] Wint.), similar results were achieved by killing the fungus selectively by heat treatment (immersing infected leaves in hot water at 50° C for 20 s), or by spraying with Ni(NO₃)₂. As a result, tissue necroses developed regularly at the infection sites in spite of the compatible host-pathogen relation (Table 2).

In summary, we can conclude that in diseases caused by *Phytophthora infestans*, *Puccinia graminis* and *Uromyces phaseoli* the hypersensitive reaction of an incompatible host is a consequence of an unknown defence reaction. This may be connected with the lack of common antigens in host and pathogens^{13,14}. It would seem that pathogens are damaged as a consequence of the host resistance and the subsequently released endotoxins are able to induce necrosis in host tissues. Necrosis, therefore, has nothing to do with the "hypersensitive type" of resistance. Brown *et al.*¹⁵ came to a similar conclusion when the area of the colony of *Puccinia graminis* was compared with the amount of necrotic tissue in incompatible wheat varieties. It is noteworthy that in several forms of plant resistance to infections hypersensitive necrosis does not occur at all^{16,17}. One can raise the question whether the term "hypersensitive reaction" of host in connexion with plant resistance has been rightly used in the past.

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Z. KIRÁLY
B. BARNA
T. ÉRSEK

Department of Pathophysiology,
Research Institute for Plant Protection,
Budapest 2, Herman O. Str. 15

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Stomatal Responses to Sulphur Dioxide

SULPHUR dioxide (SO₂) is a major air pollutant produced in industrial and urban areas, and we have studied its effect on stomatal diffusive resistance in plants.

SO₂ concentration occasionally exceeds 200 parts per hundred million by volume¹ (1 p.p.h.m. = 28.6 µg m⁻³) in industrial areas. In rural areas concentration is usually 1–10 p.p.h.m. but higher concentrations occur during short periods of fumigation from sources several miles away². Visible injury to susceptible plant tissue has been reported at SO₂ concentrations above 50 p.p.h.m.³ and at lower concentrations plant yields may be limited⁴. Gas exchange between leaf tissue and the atmosphere occurs by diffusion⁵ and is regulated by aperture changes of stomatal pores which respond to SO₂ (refs. 6, 7). Recent measurements⁷ with a viscous flow porometer showed that 25–100 p.p.h.m. SO₂ caused changes

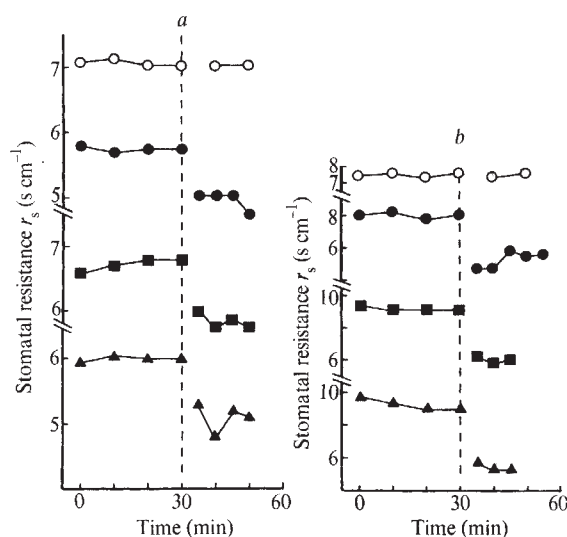


Fig. 1 Variation of stomatal resistance r_s with time and SO₂ concentration for beans (a) and maize (b). Plants were transferred to the chamber containing 0 (○), 5 (●), 10 (■) and 50 (▲) p.p.h.m. SO₂ after 30 min. Measurements were taken on the third leaf of plants at the four-leaf stage.

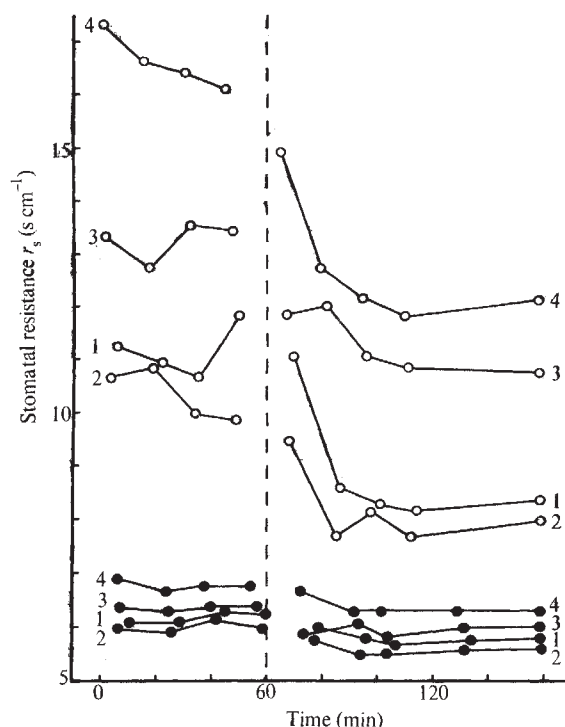


Fig. 2 Variation of stomatal resistance r_s with time for fully watered (●) and water-stressed (○) bean plants at the four-leaf stage. Plants were transferred to 10 p.p.h.m. SO₂ after 60 min. The identifying numbers 1–4 refer to the four leaves on each plant in increasing order of age.

in stomatal aperture, but there is no simple relationship between leaf resistance to mass flow and stomatal diffusive resistance.

We studied the effect of 1–50 p.p.h.m. SO₂ on stomatal diffusive resistance of beans (*Vicia faba*) and maize (*Zea mays*). Whole plants were exposed to atmospheres containing known concentrations of SO₂ in a polythene chamber. Fluorescent tubes gave an irradiance (0.3–3 µm) within the chamber of 60 W m⁻²; temperature was controlled at 20° C ± 1° C and relative humidity was 50–60%. The SO₂ air mixture was diluted from about 100 p.p.m. and flowed through the chamber at 5–10 l. min⁻¹. An identical chamber containing no SO₂ was used to monitor stomatal resistance before plants were introduced into the SO₂ chamber. The stomatal diffusive resistance, r_s , was measured with a diffusion porometer⁸ where calibration was unaffected by SO₂ up to 100 p.p.m.

The stomatal responses of beans and maize to various SO₂ concentrations are shown in Fig. 1. The plants were at the four-leaf stage and measurements were taken on the lower epidermis of the youngest fully expanded leaf of each plant. The percentage reduction in r_s was approximately constant for all SO₂ concentrations and r_s appeared to reach a minimum value independent of SO₂ concentration. This minimum did not represent full stomatal opening which was induced by lowering the carbon dioxide concentration in the chamber.

The equipment could not provide steady concentrations of less than 5 p.p.h.m. SO₂, but by uniformly increasing the SO₂ concentration in the chamber from zero, reductions in r_s at about 2 p.p.h.m. (beans) and 1 p.p.h.m. SO₂ (maize) respectively were obtained. In addition to increasing stomatal opening in light it was also possible to induce partial stomatal opening in the dark; within 2 min of transferring plants to 10 p.p.h.m. SO₂, r_s decreased from about 70 s cm⁻¹ to about 30 s cm⁻¹.

In dry conditions r_s usually increases, reducing transpiration. Fig. 2 shows the effect of 10 p.p.h.m. SO₂ on r_s of well-watered and water-stressed bean plants at the four-leaf stage. The stress was induced by withholding water from the plants for

two days before SO₂ exposure. The variation in r_s between leaves of the same plant is related to age: the younger leaves had smaller resistances. The effect of SO₂ on r_s was considerably greater on all leaves of the water-stressed plant, implying an undesirable increase in water loss. Longer term experiments showed that when plants were kept in 10 p.p.h.m SO₂ throughout a day, the increased water loss persisted, but if plants were transferred to clean air on the following day, normal stomatal behaviour was recovered.

The effect of changes in r_s on evaporation rate E can be estimated from a modified version of Penman's equation⁹ giving

$$\frac{E^*}{E} = \frac{\Delta/\gamma + 1 + r_s/r_a}{\Delta/\gamma + 1 + r_s^*/r_a}$$

where * refers to conditions with added SO₂, Δ is the slope of the curve of saturation vapour pressure of water versus temperature, γ is the psychrometric constant and r_a is the external resistance to diffusion. At 20° C $\Delta/\gamma \approx 2$ and r_a is often about 0.2 s cm⁻¹ so that for $r_s = 6$ s cm⁻¹ and $r_s^* = 5$ s cm⁻¹ (as in Fig. 1), $E^*/E \approx 33/28$, that is, the evaporation rate would be expected to increase by about 18% as a result of stomatal opening.

Our results suggest that the stomatal opening of the leaves of many crops in Britain is increased by ambient SO₂ levels. This could have two undesirable consequences: increased transpiration rate leading to early and more severe water stress in spells of dry weather, and therefore to a restriction of growth; and easier access of toxic gaseous pollutants and fungal infections to mesophyll tissue where metabolic processes may be affected. Further measurements are necessary to determine the magnitudes of stomatal responses under field conditions and the importance of vegetation in removing SO₂ from the atmosphere.

M. H. UNSWORTH
P. V. BISCOE
H. R. PINCKNEY

Department of Physiology and Environmental Studies,
University of Nottingham School of Agriculture,
Sutton Bonington, Loughborough, Leicestershire

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Receptor Site for Sour Stimuli

ELECTROPHYSIOLOGICAL studies^{1,2} showed that sour taste is induced by binding of hydrogen ions to the gustatory receptor membrane. Because biological membrane is composed mainly of protein and lipid it seems that either the carboxylic group of protein or the phosphate group of phospholipids is the receptor site for sour stimuli. We examine interaction of acids with monolayers of lipids from bovine fungiform papillae and propose that the phosphate group of phospholipids in the gustatory receptor membrane is the receptor site for sour stimuli.

Lipids were extracted from bovine fungiform papillae, which bear taste buds, with chloroform-methanol (2 : 1, v/v) and the

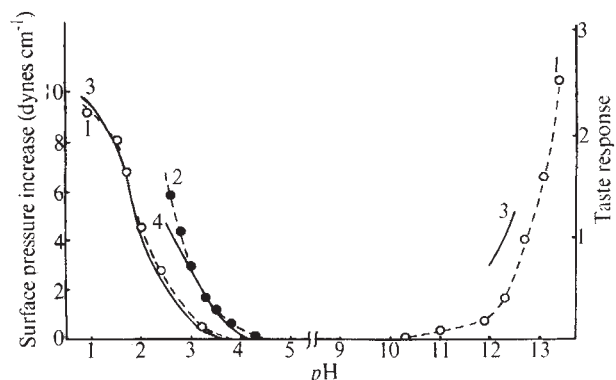


Fig. 1 Comparison of pH dependency of surface pressure increase in the lipid monolayers with that of the taste response determined by Beidler. Curve 1, surface pressure increase induced by different concentrations of HCl and NaOH. Curve 2, surface pressure increase induced by different concentrations of HAc. Curve 3, the summated response of the rat chorda tympani to different concentrations of HCl and NaOH. Curve 4, the response to different concentrations of HAc. Curves 3 and 4 were taken from Beidler's paper^{1,2}.

extracts were washed according to the method of Folch *et al.*³. Preparation and surface pressure measurements of the lipid monolayers were carried out as described in ref. 4. The interaction of acids with the monolayers was determined by measuring the increase in surface pressure in the monolayers when acids were added to the subsolution (water) under the monolayers. The initial surface pressure was 10 dynes cm⁻¹ (ref. 4).

Curve 1 (Fig. 1) shows the surface pressure increase in the monolayers when HCl and NaOH were added to the subsolution. Curve 3, which was taken from Beidler's paper^{1,2}, is a taste response curve of rat to various concentrations of HCl and NaOH. The figure shows that the surface pressure increase *v* pH curve of the lipid monolayers corresponds closely to the taste response *v* pH curve.

Sourness of acid is related primarily to the hydrogen ion concentration, but the anion also has an effect. An acetic acid solution^{1,2} (curve 4) produces a greater neural response than HCl solution (curve 3) at the same pH. Curve 2 in Fig. 1 shows the surface pressure increase in the monolayers brought about by acetic acid solution at various pHs. Acetic acid produces a larger increase in surface pressure than HCl at the same pH, which is consistent with the electrophysiological results.

Beidler¹ compared the taste responses to 0.1 M acetic acid (HAc) and 0.1 M HAc plus 0.01 M sodium acetate (NaAc), and found that the neural response to the latter was only slightly lower in magnitude than that of 0.1 M HAc, although the pH was increased from 2.88 to 3.76. Thus, although the hydrogen ion concentration decreased by a factor of seven or more, the taste response declined only slightly.

The pressure increase brought about by 0.1 M HAc was 6.5 dynes cm⁻¹, while that by 0.1 M HAc plus 0.01 M NaAc was 7.2 dynes cm⁻¹. The pressure increase by 0.01 M NaAc was only 0.8 dynes cm⁻¹. Of the increase by 0.1 M HAc plus 0.01 M NaAc, that derived from hydrogen ion binding to the lipid membrane is therefore 6.4 dynes cm⁻¹, which is close to the pressure increase by 0.1 M HAc, consistent with electrophysiological results. The acetate anion (Ac⁻) does not seem to produce any pressure increase in the monolayers; the pressure increase by NaAc was rather less than that by NaCl at equal concentration.

The larger pressure increase produced in the monolayers by HAc is due to the effects of Ac⁻, in addition to ionic strength, on the binding of the hydrogen ion to the lipid membrane. This is similar to Beidler's explanation^{1,2} for the taste response to the acids. The binding of Ac⁻ to the lipid membrane decreases the positive charge of the membrane and this causes an

increase of hydrogen ion binding to the membrane with less charge. The chloride ion binds little to the lipid membrane because of its lower affinity to lipid. Because the lipid layer in biological membrane seems to be coated with protein⁵ the influence of Ac^- binding to the lipid monolayers would be larger than that in taste response. This would explain the deviation of pressure increase ν pH curve (curve 2) at higher HAc concentration (at lower pH) from the taste response curve (curve 4). The results for pressure increase by HAc and HAc-NaAc mixture can be also accounted for by higher Ac^- concentration and larger ionic strength in HAc-NaAc mixture than those in HAc solution.

The main components of the lipid extracts from bovine fungiform papillae were cholesterol, phosphatidylcholine, phosphatidylethanolamine and sphingomyelin⁶. Therefore, the surface pressure increase in the lipid monolayers at low pH seems to come from the binding of hydrogen ion to phosphate groups of phospholipids in the monolayers. We propose that sour taste is induced by binding of hydrogen ion to a phosphate group of phospholipids in the gustatory receptor membrane.

NORIYUKI KOYAMA
KENZO KURIHARA

Biological Laboratory,
Tokyo Institute of Technology,
Meguro-ku, Tokyo

Received April 4, 1972.

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The Marmoset as an Animal Model for Teratological Research

AN extensive literature¹ exists in which rodents and rabbits have been used for the production and study of congenital malformations. There is an urgent need for the provision of a suitable near-human model for research in teratology; this became most evident when critical testing in lower mammals failed to reveal the teratogenic properties exhibited in man by thalidomide.

Recently, suggestions have been made that non-human primates² could be used for the testing of drugs and for study of the site of action of known human teratogens. The incidence of spontaneous malformations among non-human primates is low and the varieties observed are typical of those found in humans³. Wilson⁴ has prepared a method in which 30 macaque monkeys can give a reasonably comprehensive test for embryotoxicity of a drug or other chemical agent.

Macaca mulatta, *M. irus* and *Papio* spp. have been shown to be as sensitive to thalidomide as are humans⁵. These are all large animals in the adult state, expensive to house and maintain, and difficult to handle without sedation.

The common cotton-eared marmoset, *Callithrix jacchus*, is a New World monkey that has only rarely been used for laboratory investigations. It is readily available, inexpensive to purchase and house⁶ and, being omnivorous, easy to manage on a human diet. The mature marmoset weighs between 350 and 450 g, about the same weight as an adult rat; doses of rare or expensive chemicals can therefore be kept within the range that is currently used in teratological testing in rodents. Eight adult animals may be caged in the space occupied by one

adult macaque monkey. For breeding purposes marmosets are best maintained in conjugal pairs. They are exceptionally prolific breeders, with no sign of a breeding season in captivity. The quantity of foetal material available for experimental purposes is enhanced by the high incidence of multiple births. The large majority of conceptions result in twins or occasionally triplets; the gestation period is between 120 and 140 days. Employing the method advocated by Wilson⁴ for teratogenic testing in macaque monkeys of interrupting the pregnancy by hysterotomy after the end of the second trimester, six to nine foetuses can be obtained from the average female in the course of one year.

There is no obvious menstrual cycle in the marmoset as occurs in the macaque. Sexual maturity has been observed to follow demonstrable changes in the external genitalia progressing from a small penis-like vulva in the immature adult to a turgid organ with collapsing walls in the mature female. Accompanying changes occur in the dimensions and texture of the uterus, which is itself easily palpable through the thin abdominal wall. During sexual maturation the uterus becomes palpable and continues to change in width, length and consistency until, at maturity, the organ has the feel of a pencil 1.5 cm long by 0.5 cm wide. The observation of palpable dimensional change in the uterus is a consistent and reliable guide to the onset of pregnancy. Following conception the uterus changes in the first 15 to 20 days from a pencil-like to a spherical organ which is, at first, firm to hard. Regular recordings made in a colony of 30 conjugal pairs of marmosets over 2 years demonstrate that uterine diameter gives an indication of ± 2 days of the stage of pregnancy until the fiftieth day is reached. From the 15th day onwards the human Pregnosticon agglutination inhibition pregnancy test, using unconcentrated filtered marmoset urine, gives a clear positive result, much more reliable and obvious than the same test conducted on concentrated urine of pregnant macaque monkeys. The Pregnosticon latex pregnancy test does not provide a reliable sign in either species. It has not yet been possible to time pregnancies in the marmoset to within 24 h as is regularly done in *M. irus*. Daily smears of vaginal cytology have not yet provided sufficient data for determining cellular changes which may be associated with a regular cycle. In many cases, soon after parturition or hysterotomy, pregnancy has been confirmed often within 2 to 3 weeks, indicating that there is almost certainly a post-partum oestrus. The use of gonadotropin to stimulate ovulation upon withdrawal of the drugs has enabled the timing of pregnancy to within 48 h. This has assisted in the collecting of data on the developmental stages of marmoset embryos for comparison with those described by Streeter⁷ for both macaque monkeys and man.

When twin blastocysts are present in the marmoset they implant side by side in the uterus. Within about 10 days the separate chorionic walls fuse and then break down leaving twin embryos developing in separate amniotic cavities within one chorion⁸. Usually one embryo and its membranes are found attached to each of the two placental disks. When triplets are present two embryos and their membranes are attached to one placental disk and one to the other. The only two singletons observed in more than 100 hysterotomy specimens were attached to one placental disk, but two placental disks were present and connected by large vascular anastomoses. These anastomoses are present between the placental disks when both twins and triplets are conceived. No free-martinism develops and it has been shown by Benirschke and Layton⁹ that these fraternal dizygotic twins have complete immune tolerance to each other covering all organs and tissues.

One of the features of the macaque monkey which makes it of special value to the embryologist is the fact that both macaque and man, after the early somite stage, are almost identical in both the anatomical and temporal aspects of embryogenesis. A similar relationship appears to exist between marmoset and man, at least up to the stage of the beginning of closure of the posterior palate at day 47 of embryogenesis.

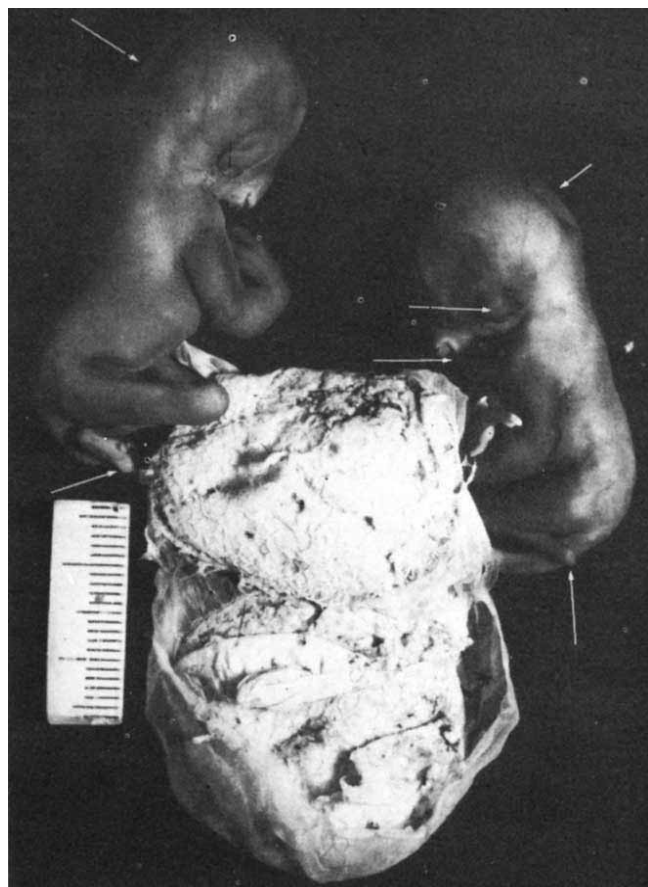


Fig. 1 Irradiated fetuses of 120 days developmental age: gross malformations marked by arrows.

Although the response of *M. mulatta*, *M. irus* and *Papio spp.* to known human teratogens such as irradiation, androgenic hormones, and thalidomide is almost identical to the response in man, it cannot yet be maintained that all agents known to be teratogenic or otherwise embryotoxic in man produce equal effects in other primates. The importance of investigating known or suspected teratogens in non-human primates is obvious; the advantages of a small primate model which parallels the teratogenic sensitivity of humans in a manner closely related to that demonstrated for the macaque and the baboon are self evident.

In order to determine the sensitivity of *Callithrix jacchus*, equivalent doses of agents known to interfere with human embryonic development have been administered.

(a) To test the effects of irradiation two marmosets at day 25 of pregnancy were subjected to irradiation carried out by a therapeutic deep X-ray apparatus operating at 200 kV and 10 mA for 4.25 min, delivering 250 rad to the uterus through the anterior abdominal wall. The remainder of the body of the animal was shielded by adequate lead protection screens. The animals were sedated by the administration of a minimal dose of intramuscular phencyclidine administered 1 h before irradiation, and they were immobilized on a bed board. Irradiation at this stage of embryogenesis produced craniorachisis, limb reduction anomalies, cleft palate, micrognathia, and abnormalities of the auricle, all of which were sufficiently gross to be observed in the hysterotomy specimen (Fig. 1). Dissection of the internal organs and microscopic examination of these fetuses have not yet been completed.

Effects of 250 r X-irradiation at day 25 of pregnancy in *Callithrix jacchus*

No. of animals	No. of offspring	No. deformed
2	4	4

(b) To test the effects of thalidomide the drug was administered to pregnant marmosets of approximately the same age and weighing approximately 400 g. Each dose consisted of 3 mg thalidomide in 3 ml. of water with 0.5% carboxymethylcellulose as a suspending agent and was administered by gastric intubation over two separate periods. Six animals were treated on alternate days, between the 25th and 35th day of pregnancy. Four animals were given similar doses on alternate days from the 35th to the 45th day of pregnancy. Each animal received a total dose of 18 mg of thalidomide equivalent to a dose of 45 mg per kg body weight. As in humans, this dose resulted in limb reduction anomalies ranging from meromelia and phocomelia to amelia, the teratogen-sensitive period being the span between the 25th and the 35th day of pregnancy. In addition, defects of the auricle and mandible similar to the oto-mandibular defects reported in man following thalidomide administration¹⁰ have also been found in marmosets.

Table 1 Response of *C. jacchus* to 45 mg/kg Thalidomide

	Dosage period	
	Day 25-35	Day 35-45
No. of animals	6	4
No. of live offspring	10	9
No. of abortions	1	0
No. of deformed offspring	11	0
No. of normal offspring	0	9

Table 1 shows the results of the administration of the drug to these two groups. Six untreated animals within the colony, subjected to a similar handling routine, were delivered of normal offspring.

The experimental data obtained from these preliminary investigations suggest that the typical appendicular defects occur when a total dose of 45 mg/kg is delivered on alternate days between the 25th and 35th day of development. The deformities are usually severe, and the upper limbs are more severely affected than the lower limbs (Fig. 2). This variation in the degree of the defect is also observed in human cases of thalidomide dysmelia. As in humans, marmoset twins and triplets manifest different degrees of deformity, perhaps supporting the belief that slight differences in maturity may significantly alter the effects of the drug (Fig. 3).

The ideal animal model for testing teratogenicity must be sensitive to all human teratogens in a predictable fashion. It appears from the experiments reported above that the common



Fig. 2 Typical limb reduction anomalies, marked by arrows, in thalidomide-treated fetus of 120 days.

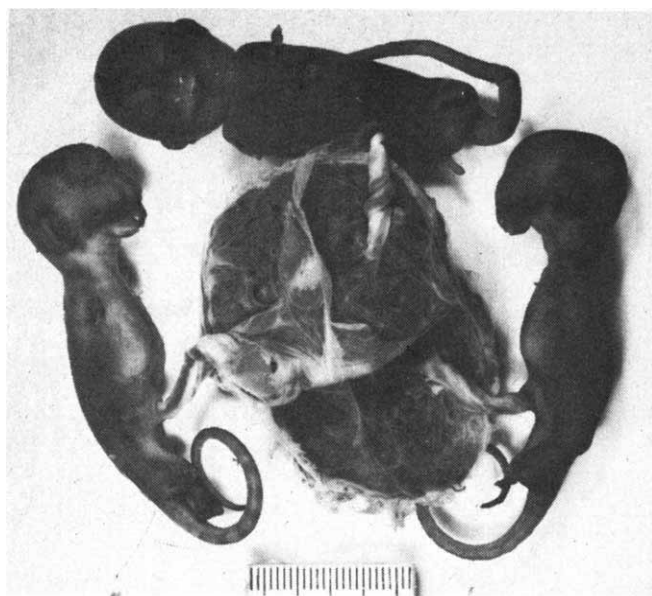


Fig. 3 Triplets showing small variations in limb and otomandibular defects after treatment with thalidomide: hysterotomy at 120 days.

cotton-eared marmoset is as sensitive to thalidomide and irradiation as is the human primate, and that the teratogen-sensitive period corresponds with that which has been described in humans. While it cannot be claimed on the basis of these preliminary investigations that the marmoset does exhibit the necessary predictability, the fact that it parallels human teratogen sensitivity does imply that it is among the most suitable non-human primate models yet available for this purpose. The marmoset may differ in menstrual and hormonal activity; also the metabolism and placental transfer of drugs may not follow the pattern obtaining in humans. For these reasons its use for the testing of new or suspected drugs may not reveal all the hazards to man from these sources; further investigation of the causal mechanism of known teratogens and the effect of viral agents such as *Rubella* would provide additional evidence as to the reliability and predictability of the model. The marmoset has the additional advantages of economy, fecundity and ease of handling which are absent in the macaque and papio species. It offers the promise of being at the very least as suitable a model for teratogenic studies as the larger and more costly non-human primates. For this reason alone it should command the attention of all concerned with the study of developmental pathology and teratology.

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D. E. POSWILLO
W. J. HAMILTON
DINAH SOPHER

Department of Dental Science,
Royal College of Surgeons of England,
Lincoln's Inn Fields, London WC2

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Experimental Induction of Foetal Malformation with "Blighted" Potato: a Preliminary Report

SEVERAL reports have linked anencephaly and spina bifida with a dietary factor related to the consumption of potatoes. Recently Renwick¹ has examined epidemiological data on anencephaly and spina bifida and postulates a link between the incidence of these neural dysplasias and the consumption of winter-stored "blighted" potatoes. He cites Leck and Record (unpublished results), who suggested that potatoes might become teratogenic as they aged, and Martin (unpublished results), who postulated that a fungus responsible for potato wart disease could be a factor in the aetiology of anencephaly and spina bifida, as there is a similarity of distributions of this disease in potatoes and neural tube defects in man. A similar association between the incidence of neural tube dysplasias and blight is thought to exist in Mexico². There have been no previous reports of a suspected teratogenic action from solanine, a human toxin^{3,4} found in stored potatoes, although animal feeding experiments have been carried out on pregnant rats⁵. No gross abnormalities of the offspring were described.

To study the effect of "blighted" potato concentrate on the morphogenesis of the mammalian embryo, a pilot study was carried out on a pure strain of Colworth Wistar rats and repeated in non-human primates of the species *Callithrix jacchus*, popularly known as the cotton-eared marmoset.

Potatoes of the variety Kerr's pink, infected with *Phytophthora infestans*, were obtained from the Agricultural Institute, Oak Park, Carlow, Eire, in December 1971 and January 1972. The solanine content of the potatoes was enhanced by covering them with brown paper and illuminating under a fluorescent domestic lighting tube for 48 h. The tubers were then washed in deionized water and sectioned into cubes for cooking. Skins, sprouts, and portions of potatoes affected by brown discoloration and softening were all included in the preparation. The raw material was then boiled in deionized water for 30 min. The cooked potato and water mixture was then homogenized, freeze dried, and stored in 1 litre sealed containers at -30°C .

The rats used had a recorded incidence of spontaneous defects of the neural tube of less than 1:1,000 (exencephaly). All does had previously produced one normal litter. They were divided into three groups comprising 6, 4, and 3 animals. Group 1 received a daily dietary mixture of 10 g of crushed freeze-dried "blighted" potato concentrate mixed with 10 g of Complian (Glaxo). Group 2 received 10 g of potato starch (BDH reagent 30262) mixed with 10 g of Complian, and group 3 received 20 g of crushed MRC 41 b pellet diet. The diets were commenced two days prior to mating; the animals were weighed every fourth day. Food consumption, estimated daily, was greatest in group 1, being slightly higher than in group 3; animals in group 2 consumed less food daily than those in the other two groups, but weight-gain graphs in all three groups were approximately the same throughout pregnancy. Throughout the experiment tap water *ad lib.* was provided for all three groups.

At day 20, after hysterectomy, embryos were examined for viability, resorption sites counted, and the embryos fixed and examined for gross malformations. The results are shown in Table 1.

The potato concentrate was also administered to marmo-

Table 1 Occurrence of Macroscopically Recognizable Defects in Rats

	Group 1	Group 2	Group 3
No. of females	6	4	3
No. of live offspring	61	25	25
No. of resorptions	0	12	1
No. of abnormal offspring	0	1 (exencephaly)	0

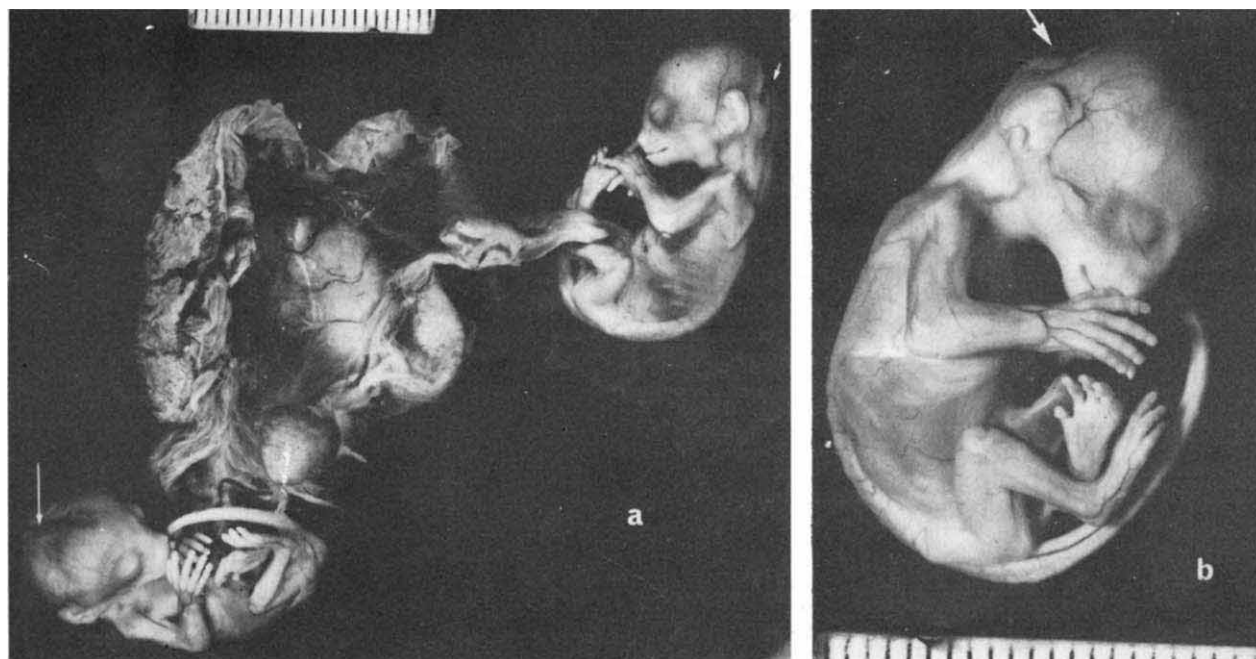


Fig. 1 Twin (a) and one of twin (b) fetuses of 80 days developmental age showing gross cranial osseous defect induced by "blighted" potato concentrate.

sets. Six pregnant females were subjected to hysterotomy in mid-December 1971. All were delivered of normal fetuses, there being five sets of twins and one singleton. Each female was housed with one male in individual cage units. From 4/1/72 until 21/2/72 each conjugal pair was given a daily dietary addition of 8 g of crushed freeze-dried "blighted" potato concentrate incorporated into the usual ration of mashed banana spread on sliced bread. A control group of six female marmosets was housed under identical conditions and fed an identical diet but without supplementary potato concentrate. The standard maintenance diet was prepared from food sold for human consumption and consisted of bread, banana, Vionate (Squibb) and hard-boiled egg. All females in this control group had previously given birth to normal offspring.

Weekly palpation of the uterus via the abdominal wall was carried out in both groups and dimensional changes believed to be associated with advancing pregnancy were recorded. Hysterotomy of all 6 females in the experimental group was carried out between 25/4/72 and 28/4/72. 11 fetuses were recovered; one set of triplets, 3 sets of twins and 2 singletons. The 11 fetuses obtained from the control females comprised 5 sets of twins and one singleton.

Study of the pregnancy records and comparison of crown-rump lengths with colony records indicated that the fetuses in both the experimental and control groups ranged in foetal age from about 80 days to about 120 days. Therefore the older fetuses in the experimental group were conceived at about the time that "blighted" potato concentrate was introduced into the diet, and the youngest fetuses were conceived about the time that dosing was discontinued.

Examination of the 11 fetuses in the control group revealed no gross abnormalities in any system. 11 fetuses in the experimental group were examined for gross anomalies, and selected fetuses chosen for histological examination. Table 2 summarizes the findings from the gross examination in the experimental and control groups. Four of the fetuses showed gross abnormalities on macroscopic examination. The most severe defects were found in those which had been conceived after the longest period of pre-conception exposure to the "blighted" potato concentrate. The oldest fetuses which had received the least pre-conception exposure showed no macroscopic evidence of abnormality.

In broad terms all defects could be classified as cranial osseous defects. In the most severe there was an obvious

defect of the occipital region of the skull above the basi-occiput and continuous with an enlarged cranial fissure (see Fig. 1). There was a noteworthy resemblance of the gross specimens to defects induced in previous experiments on the marmoset by teratogenic irradiation⁶.

Histological examination of the affected marmoset skulls has revealed that there exists, by comparison with the normal, a cranial osseous defect with replacement by collagenous membrane (Fig. 2). Beneath the area of fibrous attachment there is distortion of the underlying brain. The precise classification of the changes in the brain must await serial sectioning and detailed comparison with sections from the control specimens. These findings will be the subject of a further report.

The absence of defects in the rats fed "blighted" potato concentrate indicates that the concentrate shows neither embryolethality nor teratogenicity in the Wistar rat when supplied as a dietary supplement at the rate of approximately 30 g/kg daily throughout pregnancy.

The incidence of spontaneous defects of the cranium in *Callithrix jacchus* has not been recorded in the literature. Observations made in the marmoset breeding colony of the Royal College of Surgeons of England at Downe have revealed no spontaneous defects of any kind in 86 offspring from 36 females in the first 18 months of the colony's existence. Reliable reports on the incidence of cranial and neural dysplasia in other species of the non-human primates are difficult to obtain. After an extensive investigation, Wilson and Gavan⁷ considered that if their observations permitted any generalization, it was that "non-human primates are no more prone to spontaneous malformations than man".

The incidence of cranial osseous defects in the offspring of marmosets fed the control diet plus 10 g/kg daily of

Table 2 Occurrence of Macroscopically Recognizable Defects in Marmosets

	Experimental group	Control group
No. of females	6	6
Male offspring	8	7
Female offspring	3	4
Affected males	3	0
Affected females	1	0

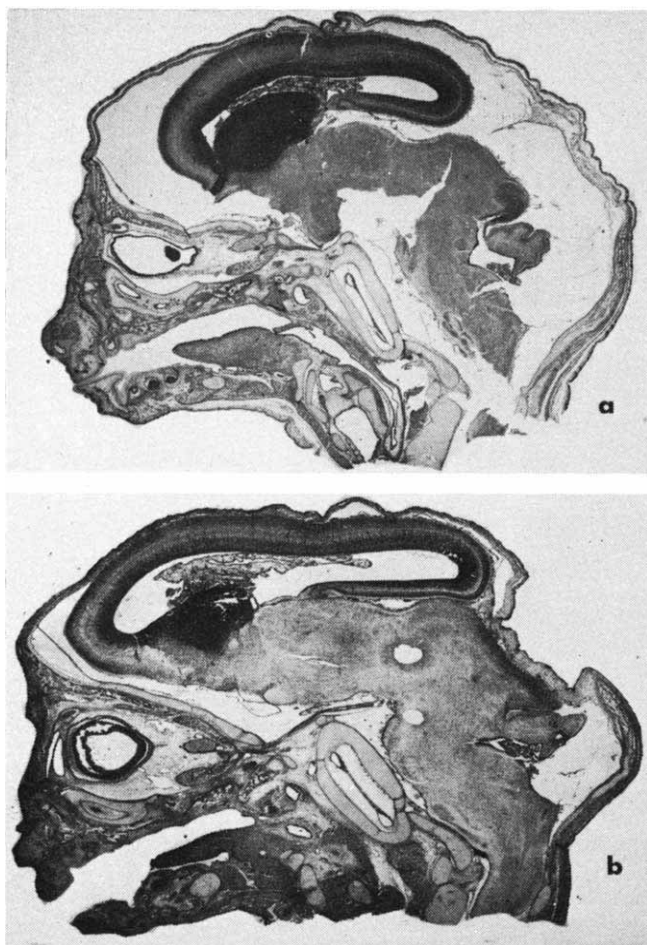


Fig. 2 Sagittal section of head and control foetus of 80 days (a) compared with equivalent section of affected foetus (b) showing histological appearance of cranial dysplasia found in foetus shown in Fig. 1b.

"blighted" potato concentrate is therefore all the more striking. The severity of the defect appears to be related to the length of pre-conception exposure of the conjugal pair to the experimental diet. Further experiments are in progress designed to investigate controls in which a concentrate prepared from normal potatoes is being fed under comparable conditions. It is also hoped that it will be possible for affected marmosets to be born at term, and that their post-natal growth and development can be observed to provide further information on the precise effects of the cranial deformity.

It is obvious that the "blighted" potato concentrate which has exhibited teratogenicity in this species of non-human primate contains a variety of chemical compounds, any or all of which may, singly or in combination, act on the developing embryo in the first trimester to produce cranial dysplasia. The causal agent or agents may act in a multifactorial fashion and *C. jacchus* may be a particularly sensitive primate model for the evaluation of such teratogenicity.

The teratogenicity of the concentrate has been demonstrated in the marmoset; selective teratogenic testing of compounds extracted from the "blighted" potato should prove no more difficult to carry out in this species. The identification of a causal agent or causal mechanism of malformation in a species other than man however does not provide grounds for extrapolating results directly from the animal to man. If the defect in both the animal model and man is similar, and can be shown to follow a common developmental pathway, even in part, extrapolation becomes a possibility. This preliminary communication suggests a causal relationship between compounds found in the "blighted" potato and the appearance of cranial osseous defects in a non-human primate. Further

investigations may permit this extrapolation to be extended.

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D. E. POSWILLO
DINAH SOPHER
SANDRA MITCHELL

Department of Dental Science,
Royal College of Surgeons of England,
Lincoln's Inn Fields, London WC2

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Classification and Evolution of Ornithopod Dinosaurs

ALTHOUGH I agree with the main themes of Thulborn on the origin of ornithischian dinosaurs¹⁻³, I disagree⁴⁻⁶ with the current classification of the family Hypsilophodontidae⁷⁻⁸, in respect to premaxillary teeth as a diagnostic feature, cheeks, cheek teeth and locomotor capabilities.

The presence of premaxillary teeth is used as a diagnostic feature in all discussions of the family Hypsilophodontidae. The following genera of lower ornithopods have premaxillary teeth: *Fabrosaurus*², *Heterodontosaurus*, *Echinodon*, *Hypsilophodon* and *Thescelosaurus*. Thulborn¹⁻³ refers all these genera (with *Heterodontosaurus* as *Lycophinus*) to the family Hypsilophodontidae, but premaxillary teeth are also present in the family Pachycephalosauridae (*Stegoceras*) and in the sub-orders Ceratopsia (*Protoceratops*, *Montanoceratops*) and Ankylosauria (*Silvisaurus*)⁹. As *Dysalotosaurus* lacks premaxillary teeth it was removed^{10,11} from the Hypsilophodontidae to the Iguanodontidae^{1-3,12}, but premaxillary teeth have been independently lost in at least three lines (ornithopods, ceratopsians, ankylosaurs) and could have been lost within the family Hypsilophodontidae. The ancestral thecodont had premaxillary teeth but the presence or absence of these teeth cannot be used to determine which ornithopod genera should be referred to the family Hypsilophodontidae.

Fabrosaurus and *Echinodon* are the only ornithischians resembling living reptiles in having marginally positioned maxillary and dentary teeth¹³. I consider that the retention of this primitive character justifies the placing of these genera in a new family, the Fabrosauridae. In all other ornithischians these teeth are inset, with a space lateral to the maxillary and dentary tooth rows overhung by the maxilla and floored by the massive dentary. I correlate marginal teeth with the absence of cheeks, and an inset dentition with the presence of cheeks¹³. In most ornithischians the mouth was small and the space lateral to the tooth rows was delimited by a cheek^{6-8,13} that was attached to the longitudinal ridges on the maxilla and dentary. The skulls of ornithischians show many specializations for a herbivorous diet and, as shown by the pattern of tooth wear, ornithischians ate resistant plant material which must have been chewed repeatedly. The cheeks of ornithischians were not homologous with the cheeks (*M. buccinatoris*) of mammals, a derivative of the facial musculature, but were probably a derivative of the superficial trigeminal musculature with the same function: to prevent the loss of food from the sides of the jaws that otherwise would

inevitably have occurred during chewing¹³. I attribute the spectacular success of ornithischians, the dominant "small to medium" sized (up to 10 m) terrestrial herbivores during the Jurassic and Cretaceous periods (about 115 m.y.) to the development of cheeks within the group¹³.

In hypsilophodonts the obliquely inclined wear surfaces of adjacent cheek teeth are not all in the same plane¹³⁻¹⁵, but in *Heterodontosaurus* they are, and there is a caniniform tooth on each premaxilla and dentary^{5,14,15}. These dental specializations of *Heterodontosaurus* justify the retention of the family Heterodontosauridae.

In cursorial animals the distal part of the slender limb is elongate with a very rapid stride while in graviportal animals the distal part of the much more massive limb is short with an ambling gait¹⁶. Ornithopods can be divided into these two types on the basis of the proportions of the hind limbs (Table 1, Fig. 1). In cursorial ornithopods the ratio of the length of the hind limb to the trunk is about 1.5 : 1; the tibia is longer than the femur; the third metatarsal is more than half the length of the femur; and the ratio of the combined lengths of the tibia plus metatarsal III to the femur is more than 1.6:1; a value well within the range for cursorial mammals^{7,8,16}. The postcranial proportions of *Dysalotosaurus* are very similar to those of *Hypsilophodon*^{7,8,17} (Table 1) and *Dysalotosaurus* was undoubtedly cursorial. In addition, the skull is very similar to that of *Dryosaurus*, a contemporaneous hypsilophodont from North America (work in progress). *Thescelosaurus* is considered to be a hypsilophodont, but most of the characters of the sub-family Thescelosaurinae¹⁸ reflect the graviportal adaptations of *Thescelosaurus* which, in its proportions, resembles the iguanodonts *Camptosaurus* and *Tenontosaurus*¹⁹ much more closely than it does any hypsilophodont (Table 1). I consider that the family Hypsilophodontidae should be restricted to cursorial forms including *Dysalotosaurus* and I refer *Thescelosaurus* to the family Iguanodontidae.

The boundary between cursorial hypsilophodonts and graviportal iguanodonts was probably crossed several times and, although actual relationships cannot be established at the moment, at least three lines were involved²⁰ (Fig. 1). The family Iguanodontidae is a horizontal grade for all graviportal ornithopods which lack the cranial specializations of hadrosaurs and pachycephalosaurs. The morphological variation within the family ranges from close to the ancestral hypsilophodonts (*Thescelosaurus*) to close to the hadrosaurs which evolved from a line close to *Iguanodon*²¹. Hadrosaurs were probably bipedal while running²² but I did not demonstrate that hadrosaurs show some cursorial adaptations as cited by Thulborn¹; the hind limbs of hadrosaurs were clearly adapted for graviportal locomotion²² (*Kritosaurus*, Table 1) although at times they probably moved quite rapidly as do elephants. Another graviportal derivative of the cursorial hypsilophodonts were

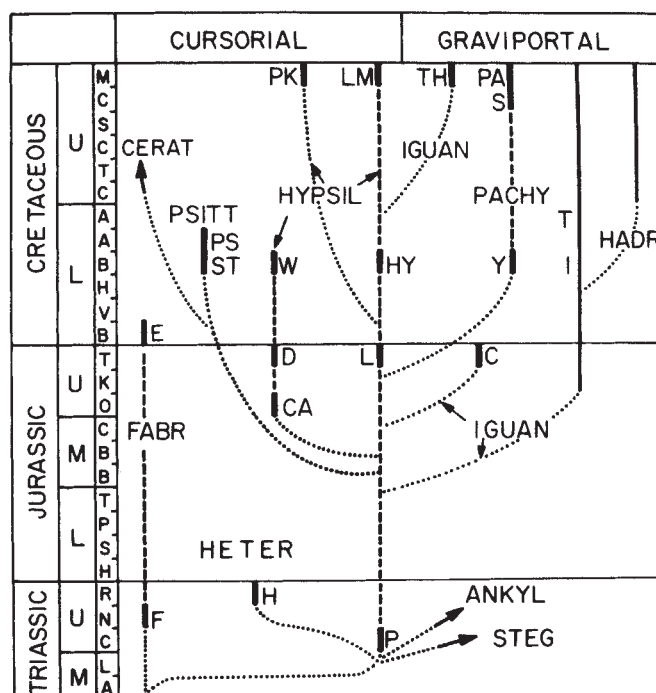


Fig. 1 Phylogeny of the Ornithopoda. Diagram to show the phylogenetic relationships and the nature of the fossil record of lower ornithopods. The ages of the different genera are based on data in Charig²⁷ and the stratigraphic distribution is by stages, the initials of which are given in the third column. Abbreviations: Classification units—ANKYL, Ankylosauria; CERAT, Ceratopsia; FABR, Fabrosauridae; HADR, Hadrosauridae; HETER, Heterodontosauridae; HYPsil, Hypsilophodontidae; IGUAN, Iguanodontidae; PACHY, Pachycephalosauridae; PSITT, Psittacosauridae; STEG, Stegosauria. Genera: C, *Camptosaurus*; CA, "*Camptosaurus*" leedsi; D, *Dryosaurus* and *Dysalotosaurus*; E, *Echinodon*; F, *Fabrosaurus*; H, *Heterodontosaurus*; HY, *Hypsilophodon*; I, *Iguanodon*; L, *Laosaurus*; LM, "*Laosaurus*" minimus; P, *Pisanosaurus*; PA, *Pachycephalosaurus*; PK, *Parksosaurus*; PS, *Psittacosaurus*; PSITT, *Psittacosaurus*; ST, *Stegoceras*; TH, *Thescelosaurus*; W, *Wealden hypsilophodont* (undescribed, work in progress); Y, *Yaverlandia*²². —, Actual fossil record for lower ornithopods; ---, no fossil record but genera in the same vertical line are closely related;, postulated relationships.

the pachycephalosaurs (*Stegoceras*, Table 1) which probably ran like other ornithopods with the back held horizontally²² and used the massive skull cap as a battering ram for intra-specific competition as do goats and sheep^{6,23}.

I present a classification of cursorial ornithopods, in which the families are presumed to have been cursorial, with a skull usually small with a proportionally short snout and a large orbit, teeth in a single row (for post-cranial proportions see above and Table 1).

Family Fabrosauridae, nov. fam. Maxillary and dentary teeth marginal, flat maxilla, dentary slender, rudimentary anterior process to pubis^{1,3}; *Fabrosaurus*, *Echinodon*. The skull of *Fabrosaurus* is very primitive^{3,13} and probably almost identical to the somewhat earlier form (?Middle Triassic) from which all ornithischians were ultimately derived (Fig. 1).

Family Hypsilophodontidae. Maxillary and dentary teeth inset (longitudinal recess to maxilla, massive dentary) with randomly formed wear surfaces that are not all in the same plane; no canine teeth or large rostral beak; *Pisanosaurus*, *Tatisaurus*, *Nanosaurus*, *Laosaurus*, *Dryosaurus*, *Dysalotosaurus*, *Hypsilophodon*, *Parksosaurus*. Cheeks were present in this family which represents the central stock of persistently conservative cursorial ornithopods from which most other ornithischians were derived either directly or indirectly (Fig. 1). Thulborn¹ visualized the succession of hypsilophodonts as an evolutionary plexus in which each genus represents an individual strand without any genera being directly related. I consider

Table 1 Hind Limb Ratios of Ornithopod Dinosaurs⁷

	F	T	T+MT	MT	MT	HL
	in mm	F	F	F	T	TR
Cursorial genera						
<i>Fabrosaurus</i>	104	1.24	1.88	0.64	0.52	1.54
<i>Hypsilophodon</i>	151	1.18	1.73	0.56	1.47	1.53
<i>Parksosaurus</i>	270	1.18	1.74	0.56	0.47	1.50
<i>Dysalotosaurus</i>	366	1.10	1.69	0.59	0.54	1.44
<i>Dryosaurus</i>	353	1.10	1.64	0.52	0.47	—
<i>Psittacosaurus</i>	162	1.10	1.68	0.57	0.52	1.30
Graviportal genera						
<i>Camptosaurus</i>	242	0.95	1.31	0.40	0.42	1.20
<i>Tenontosaurus</i>	355	0.95	1.35	0.39	0.40	1.09
<i>Iguanodon</i>	678	0.88	1.23	0.35	0.40	1.24
<i>Thescelosaurus</i>	355	0.83	1.20	0.36	0.42	1.08
<i>Kritosaurus</i>	1,045	0.90	1.24	0.35	0.38	1.27
<i>Stegoceras</i>	222	0.99	1.33	0.38	0.38	—

Abbreviations: F, femur; HL, hind limb (=F+T+MT); MT, metatarsal III; T, tibia; TR, trunk (combined lengths of the centre of the dorsal vertebrae).

that some relationships are discernible within this family (ref. 21 and work in progress) (Fig. 1). All hypsilophodonts were probably derived from a form similar to *Pisanosaurus*²⁴. *Laosaurus celer* Marsh 1878 is very similar to *Hypsilophodon foxii* Huxley 1869 and these two species may prove to be congeneric. In the Upper Cretaceous the *Laosaurus*–*Hypsilophodon* line is represented by "*Laosaurus*" *minimus* from North America²⁰. *Dryosaurus altus* Marsh 1894 is very similar to *Dysalotosaurus lettow-vorbecki* Pompeckj 1920; these two species may also prove to be congeneric. Femora of the *Dryosaurus*–*Dysalotosaurus* type are also found in England in the Upper Jurassic ("*Camptosaurus*" *leedsi*)²⁵ and in the Lower Cretaceous (Wealden hypsilophodont) (work in progress). *Parksosaurus* resembles the *Laosaurus*–*Hypsilophodon* line more closely than the *Dryosaurus*–*Dysalotosaurus* line in the form of the femur and the elongate nature of the frontals²⁰. *Tatisaurus*²⁶ and *Nanosaurus* may not be hypsilophodonts.

Family Heterodontosauridae. Inset cheek teeth with planar wear surfaces, large caniniform tooth on premaxilla and on dentary, rudimentary anterior process to pubis⁵; *Heterodontosaurus*, *Lycorhinus*, *Geranosaurus*.

Family Psittacosauridae. High, broad and relatively large skull, deep beak formed by rostral bone, inset cheek teeth with randomly formed wear surfaces, long anterior process to pubis; *Psittacosaurus*, *Protiguanodon*, ?*Stenopelix*.

P. M. GALTON

Department of Biology, University of Bridgeport,
Bridgeport, Connecticut 06602

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Heterologous Reinnervation of the Iris from Sympathetic Ganglia in Organ Culture

AFTER division of a sympathetic nerve, reinnervation occurs by growth of axons from neurones located in the sympathetic ganglia. These ganglia survive in artificial media, and the neurones continue to respond to acetylcholine for more than 1 week after their isolation *in vitro*¹. A rat iris placed in contact with a superior cervical ganglion in organ culture is ramified by nerve fibres emanating from the ganglion cell bodies². The catecholamine histochemical method has shown that nerve fibres begin to enter the iris from the ganglion as early

as the second day in culture in the presence of exogenous nerve growth factor².

To study the selectivity of the process of reinnervation, heterologous matchings of iris and ganglia in culture were examined. We report here cross-species ramification of neuronal fibres from ganglia of the rat, mouse and guinea-pig into heterologous irises.

Adult male Sprague-Dawley rats (175–200 g), NIH mice (30 g) and Hartley guinea-pigs (700 g) were killed by cervical dislocation. The superior cervical ganglia and irises were removed aseptically, placed in ice-cold medium, and organ cultures were prepared as described previously². Each iris was stretched over a grid made of minimesh metal (1.4 mm mesh; the Expanded Metal Company Ltd, West Hartlepool, England) with the superior cervical ganglion of the same or other species overlapping its cut margin. The grids were placed in vessels containing a chemically defined medium, BGJ_B (Grand Island Biological Co.), supplemented with nerve growth factor (100 U/ml)³ (Burroughs Wellcome Co., Research Triangle Park, North Carolina) and incubated in an atmosphere of 95% oxygen–5% carbon dioxide at 37° C.

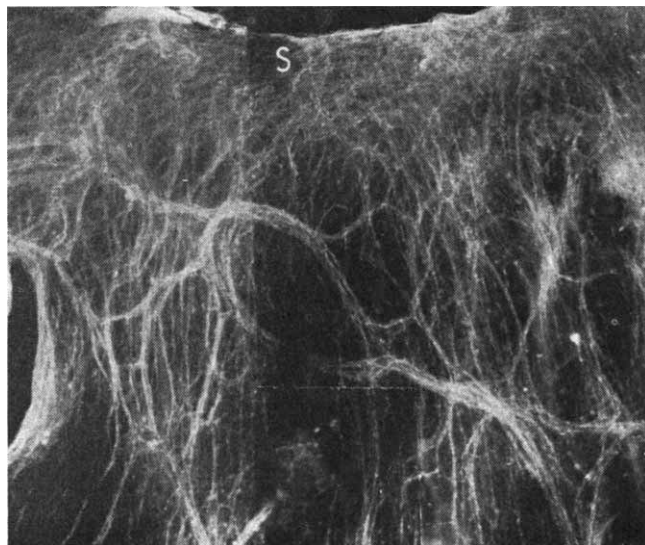


Fig. 1 Rat iris cultured with a mouse superior cervical ganglion. Adrenergic nerve plexus containing large preterminal trunks and fine varicose processes. The sphincter region (S) is below the pupil ($\times 50$).

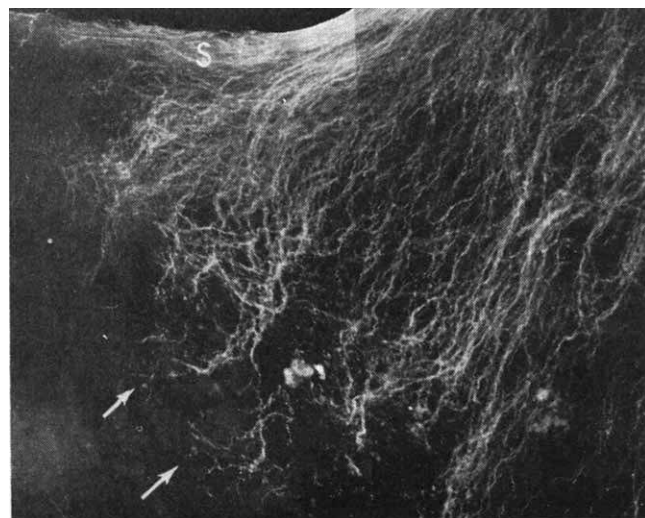


Fig. 2 Guinea-pig iris cultured with a rat superior cervical ganglion. An abundant number of adrenergic nerves are observed behind the advancing front of ramifying nerve fibres (arrows). Sphincter region (S) is below the pupil ($\times 50$).

After 6 days in culture, the irises were washed in balanced salt solution, treated with paraformaldehyde fumes and examined under the fluorescence microscope⁴.

The nerve processes that emanate from the superior cervical ganglion of the rat successfully ramify in the iris of the same species in organ culture². The reinnervated iris regains the ability to concentrate catecholamines and regains the enzyme, dopamine- β -hydroxylase (DBH), which converts dopamine to noradrenaline⁵. The ingrowth process depends on nerve growth factor, a protein distributed among many species of animals, including man³. A recent report has suggested that nerve growth factor may be essential for adrenergic reinnervation of tissues in the central nervous system⁶.

An extensive nerve plexus was found in the rat iris cultured with the mouse superior cervical ganglion; the nerve plexus extended from more than a quarter to a half of the iris (Fig. 1). There were more nerves present than normally seen in the rat iris. Abundant fine varicose fibres (Fig. 3) and numerous large preterminal trunks were dispersed throughout the iris

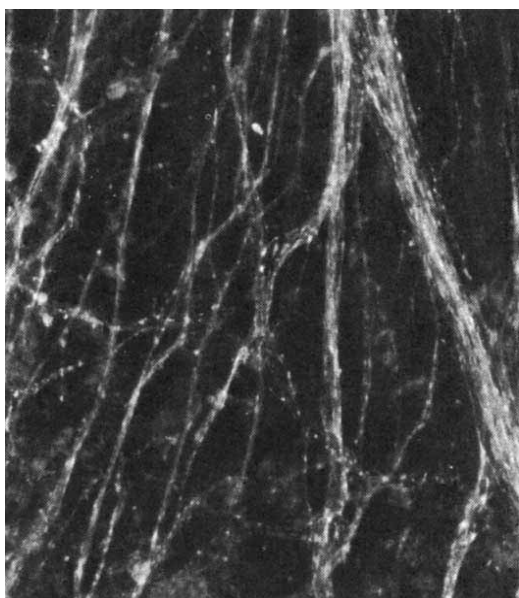


Fig. 3 Rat iris cultured with a mouse superior cervical ganglion. Fine varicose nerve fibres and large processes containing both smooth and varicose adrenergic nerves ($\times 240$).

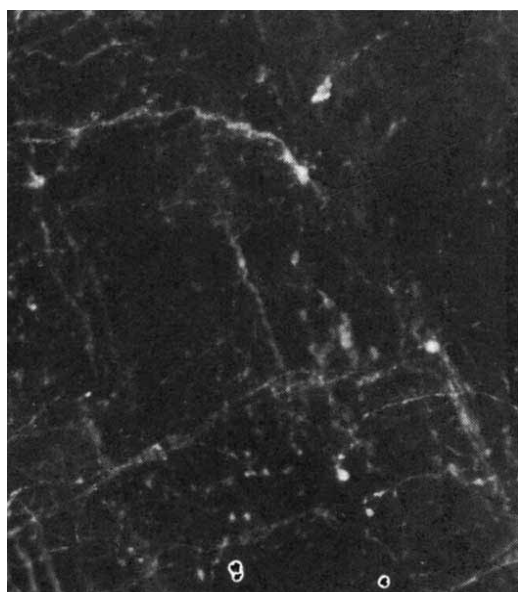


Fig. 4 Guinea-pig iris cultured with a rat superior cervical ganglion. Mostly fine smooth processes with few varicosities are observed ($\times 240$).

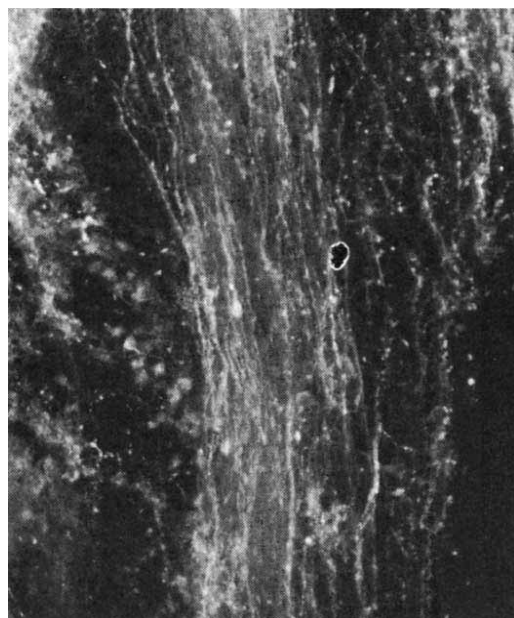


Fig. 5 Mouse iris cultured with a rat superior cervical ganglion. Many smooth and varicose adrenergic fibres ($\times 152$).

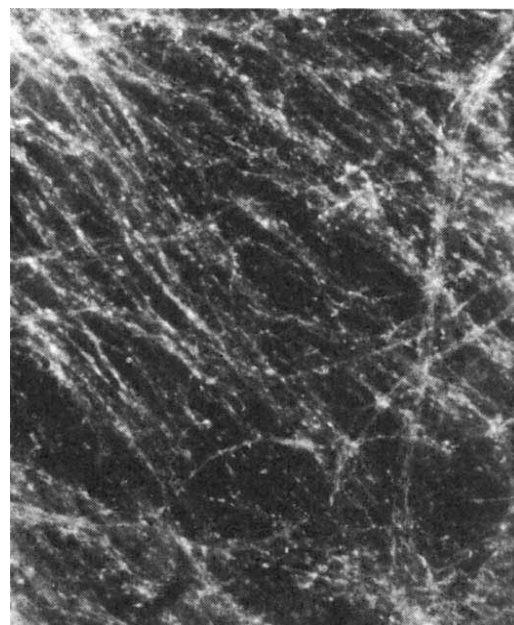


Fig. 6 Guinea-pig iris cultured with a mouse superior cervical ganglion. Abundant varicose and smooth nerve fibres are observed ($\times 384$).

preparation (Figs. 1 and 3). When cultured with the guinea-pig iris, the rat superior cervical ganglia produced a large number of moderate to intensely fluorescent nerves which extended from a third to three-quarters of the iris, including the entire radius (Fig. 2). The nerves were mostly fine smooth processes with only a few varicose fibres present (Fig. 4). When we attempted to culture the rat ganglion with the mouse iris, the preparation tended to be distorted because of the great difficulty in handling the small fragile mouse iris. In spite of the difficulty, abundant nerves could be observed after 6 days of culture. Some regions of the iris contained an extensive overgrowth of nerves with many smooth and varicose fibres (Fig. 5); many nerves contained large fluorescent swellings which were probably growth cones. Nerves from mouse ganglia could also successfully ramify in the guinea-pig iris and produce abundant fine nerve fibres which were both smooth and varicose (Fig. 6).

This study demonstrates that ganglia taken from one rodent

species can reinnervate the iris of another species. Olson and Malmfors⁷ showed that the mouse superior cervical ganglia, transplanted to the anterior chamber of the rat eye, could successfully innervate the rat denervated iris. Our results suggest that the factors such as nerve growth factor, which are responsible for the formation of neural networks, are similar in different species. An alternative explanation is that nerve fibre ramification is relatively unspecific. Sympathetic nerves in organ culture, however, do not ramify in tissue not normally innervated such as the diaphragm or sclera (unpublished observations of S. D. S.).

Very little is known about the factors responsible for reinnervation of peripheral tissues or synapse formation in the central nervous system. It has been suggested that glial and Schwann cells may play a role in the process of directing nerve fibres to their destination⁸. In peripheral tissues, the pattern of nerve fibre ramification may be determined by the tissue being reinnervated⁷. Organ culture systems can serve as useful tools in research on the control of the direction of nerve fibre growth.

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STEPHEN D. SILBERSTEIN*
KENNETH R. BERV
DAVID M. JACOBOWITZ

Laboratory of Clinical Science,
National Institute of Mental Health,
Bethesda, Maryland 20014

Received June 22, 1972.

* Present address: Department of Neurology, University of Pennsylvania Hospital, 34th and Spruce Streets, Philadelphia, Pennsylvania 19104.

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"Dished Faces", Brain Size and Early Hominids

THE cranium of Olduvai hominid 24 has been claimed by M. D. Leakey, Clarke and L. S. B. Leakey¹ to represent a member of the genus *Homo*, possibly a female *Homo habilis*. An unsigned comment in *Nature*, headed "Confusion Over Fossil Man"², questions this attribution and claims that the new cranium does not fulfil all the generic criteria outlined in the definition of *Homo* proposed by Leakey, Tobias and Napier in 1964³. In particular, it is claimed that Olduvai hominid 24, first, does not possess the minimum required cranial capacity of "about 600 cm³" and, second, possesses a "dished" face such as characterizes many known australopithecine crania but not *Homo*—on the forementioned definition.

I should like to show here that this comment is erroneous in both respects, and that the cranium of OH 24 is therefore not disqualified from membership of the genus *Homo* as defined.

In the revised diagnosis of *Homo*³, the following criteria related to the cranial capacity: "the cranial capacity is very variable . . . [it] is (on the average) large relative to body-size and ranges from about 600 cc in earlier forms to more than 1,600 cc". The capacity of OH 24 yielded preliminary determinations of 560 cm³ (ref. 1). It should be noted that this value was based on a determination, not an estimate. A plaster endocast of the reconstructed cranium of OH 24 was made by Mr R. J. Clarke and its volume was determined by water displacement by Mrs Margaret Leakey and Dr A. Walker. The mean of six such

determinations came to 567 cm³. It is an arguable point, and might be deemed by some to be a quibble, whether a capacity "in the order of 560 cm³" qualifies to be included under the formula "about 600 cm³". The mean cranial capacity of *Australopithecus africanus* is of the order of 450 cm³ and the sample range is 428 to 485 cm³ (ref. 4), so that it might well be considered that 560 cm³ is "about 600 cm³". Even the robust australopithecines have cranial capacities of 500 to 530 cm³ (n=3); thus the cranial capacity of every proven *Australopithecus* is exceeded by the value 560 cm³.

More important, though, is the fact that about 560 cm³ is the capacity of the calvaria in its present rather crushed state—it might be said to be the "taphotype" but not the phenotype of OH 24. Leakey *et al.*¹ stressed that the reconstruction of the cranium has left it with some imperfections: "The base of the cranium is also still depressed into the brain cavity"; and "The crushing of the whole cranium has also to be taken into account when considering the cranial capacity, which must inevitably have been greater than the absolute capacity as measured now" (ref. 1, p. 310).

In my study of OH 24 for the fifth volume of *Olduvai Gorge*, I have noted that from the region of the lambda behind to that of the metopion in front, the calotte or roof of the calvaria shows a fracture-depression downwards and into the upper part of the brain case. Furthermore, a large part of the cranial base is stove inwards, particularly the posterior cranial fossa. Some reduction in the measurable cranial capacity must certainly follow. Thus, both on the roof and the floor of the endocranial cavity, damage to the lining bones tends to reduce the measurable capacity. The value of 567 cm³ is uncorrected for the cranial distortion: an estimate of the capacity on restoration gives a value of the order of 600 cm³ (Tobias⁴, p. 80). Indeed, the capacity may even have exceeded 600 cm³. Dr R. L. Holloway has independently made a restoration of the endocast and has kindly permitted me to say that this yields a capacity of some 590 cm³. Hence, in this respect, OH 24 qualifies to be included in the genus *Homo*.

The other supposed disqualification of OH 24 from membership of *Homo*, according to *Nature's* commentator², is that OH 24 has a dished face. Clearly the author of this comment has misunderstood the way in which the term "dished face" has been used in the palaeoanthropological literature. In the diagnosis of *Homo*, Leakey *et al.*³ were at pains to state that though the concave (or dished) face "is common in members of the Australopithecinae", it certainly does not characterize all australopithecines. By stating that the "dishing" can be detected in a profile drawing, this commentator displays the mistaken belief that "dishing" (as applied to early hominid crania) refers to a hollowing from above downwards, that is from glabella to the front teeth in the median plane. This is not the way in which the term has been used in the literature on the Australopithecinae^{3,5-9}.

It seems that Professor J. T. Robinson first used the term to describe the facial architecture of *A. robustus* ($\equiv$ *Paranthropus*), in contrast with that of *A. africanus*. A striking feature of the robust australopithecines is the facial flattening from side to side. This is the usual sense in which the term "facial flattening" is used, as for instance in the classical study of facial flatness by Woo and Morant¹⁰. This marked facial flattening in *A. robustus* is partly a result of the decreased relative size of the anterior teeth and of the naso-frontal buttress, but is chiefly the consequence of the greatly increased robustness of the zygomatico-maxillary buttress, attendant on exaggerated size and function of the cheek teeth.

Sometimes, indeed, the face is hollowed between one cheek bone and the other, so that "the nasal region is set in a central facial hollow"¹¹. It is to this side-to-side hollowing that Robinson applied the term "dished"⁸. Subsequently he incorporated this term into his revised diagnosis of *Paranthropus*: "Bony face either quite flat or actually dished"; but not into that of *Australopithecus* (in the restricted sense of Robinson): "Bony face moderately—but not completely—flat"^{7,9}. In this sense,

which is the sense used in the revised diagnosis of *Homo*³, dishing applies only to the robust australopithecines (members of the superspecies *A. robustus/A. boisei*¹¹), but not to *A. africanus*. If the commentator's interpretation were correct, then clearly the face of *A. africanus* would be much more dished, and that of the robust australopithecines hardly dished at all! The face of the robust australopithecines is in fact flat to hollowed from side to side, and is very slightly hollowed from above downwards; that of other early hominids is flattish to slightly convex from side to side, but is appreciably hollowed from above downwards.

The commentator's use of "dishing" is perhaps based on the usage of dog breeders and stock breeders for whom "dishing" applies to a hollowing from forehead to teeth (*teste* Dr M. D. Leakey and Mrs R. Walker).

In view of the confusion which has arisen, it would perhaps be better if the term "dishing" were not used in palaeoanthropology in the sense in which Robinson first used it and as has been followed by others, or in the sense followed by the commentator! Instead, it would be less confusing to adhere to the wording proposed in the diagnosis of *A. robustus* and *A. boisei*, namely that "the nasal region is set in a central facial hollow"^{11,12}. Meantime, on the usage of Leakey *et al.*³ in their revised diagnosis of *Homo*, it would be correct to say that OH 24 does not show a "dished" face, and that it is not thereby disqualified from membership of the genus *Homo*.

One last point; the commentator implied that because the cranial capacity of OH 24 cannot be determined with accuracy, the validity of many of the cranial measurements might be undermined, and therefore that of the conclusions drawn, at least in part, from those measurements. A close study of the cranium and its distortion reveals that relatively few of the uncorrected measurements are so affected, and further, that the diagnosis of the taxonomic affinities does not depend on any of those measurements, save for cranial capacity which has been dealt with before.

I conclude that if the criteria proposed for *Homo* by Leakey *et al.*³ are valid—and they differ from those of Le Gros Clark¹⁴ principally in respect of cranial capacity—then it is reasonable to assign OH 24 to *Homo*. This inference is supported by such additional features as the wide spacing of the anterior parts of the temporal crests, the very small absolute size and the shape of the palate and the extreme forward position of the foramen magnum on the base of the cranium, as compared with its position in such australopithecine crania as Sterkfontein 5, Makapansgat 37/38, OH 5 (the former "*Zinjanthropus*") and KNM-ER-406 of Ileret, East Rudolf.

It is not unreasonable to conclude that OH 24 adds powerfully to the growing body of evidence from Olduvai, East Rudolf and elsewhere that an early member of the genus *Homo* existed alongside australopithecines in the Lower Pleistocene.

I thank Dr M. D. Leakey, Mr R. E. F. Leakey, Mrs Margaret Leakey, Dr A. Walker, Mrs Pat Barrett, the Kenya National Museum, Centre for Prehistory and Palaeontology, and the Research Committee of the University of the Witwatersrand, Johannesburg.

PHILLIP V. TOBIAS

Department of Anatomy, University of the Witwatersrand, Hospital Street, Johannesburg

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Reply to Professor Tobias

THROUGH the kindness of the Kenya National Museum I have had the opportunity of examining the original material of OH 24 in Nairobi on several occasions. I am therefore in a position to reply to Professor Tobias's letter concerning this important specimen.

It is vital not to underestimate the amount of post-mortem crushing and distortion still present in OH 24. Although a skilful job of reconstruction has been done, at least two areas of gross distortion remain (perhaps unavoidably), and it is imperative that these areas of distortion are not misinterpreted when making taxonomic considerations. In this regard, the most crucial source of distortion occurs in the occipital bone. There are multiple fractures in the occipital and a major fracture runs laterally through the internal occipital protuberance, 15.5 mm posterior to opisthion, causing inferior and distal displacement of the posterior fossae above the line of breakage. This has had the effect of making the skull appear longer than it should be and a correction of this distortion will decrease the cranial capacity as published.

Further distortion occurs in the frontal region where severe downward crushing after burial displaced the frontal bone both anteriorly and inferiorly. After reconstruction, the supra-orbital portion of the frontal is "floating" freely on the left and the join with the orbital portion of the zygomatic, on the right, is uncertain. In my opinion, therefore, the supra-orbital area of the frontal could be moved in a slightly superior and posterior direction. This would also have the effect of shortening the cranium and thereby diminish the cranial capacity still further.

Professor Tobias's discussion of what constitutes facial "dishing" in *Australopithecus* seems irrelevant; surely it is the occurrence of this feature in a purported member of the genus *Homo* which is under consideration and not its admitted presence in *Australopithecus*. The original definition¹ referred to a "concave" or "dished" face, although Professor Tobias may prefer the term "central facial hollow". A concavity is defined as "having the outline or surface curved like the interior of a circle or sphere"². Since circles or spheres have, by definition, no up or down or side to side, Professor Tobias's qualification regarding a side to side concavity is meaningless. Either a structure is concave (or "hollow") in all directions or it is not concave. May we now conclude that either the specimen does not fit the definition or that the definition does not fit the specimen?

Semantic disputes aside, one of the most interesting aspects of the morphology of OH 24 is the remarkably close similarity this specimen shows with MLD 6, an australopithecine from Makapansgat, South Africa. Although MLD 6 is a fragmentary specimen, the preserved portion of its central facial area shows a near identity with the comparable portions on OH 24, not only in size and shape but in detailed morphology. Moreover, the teeth which can be compared, PM⁴ and M¹, although showing different degrees of wear, are extremely similar in their measurements, relative sizes and general conformation.

Finally, I must entirely agree with Professor Tobias that there is growing, and perhaps incontrovertible, evidence of the existence of a more advanced hominid in the early Pleistocene parts of Africa. I must, however, emphatically deny that "Twiggy" is part of this evidence.

Received September 11, 1972.

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BOOK REVIEWS

Unobjective Science

Civilization and Science in Conflict or Collaboration? (A Ciba Foundation Symposium.) Pp. viii+227. (Elsevier: Amsterdam, London and New York; Excerpta Medica, North Holland: Amsterdam, London and New York, 1972.)

IN addition to their purely scientific discussion meetings the Ciba Foundation have held several symposia on more general matters of concern to the scientific establishment. This newest in the series discusses the crisis (or non-crisis—take your pick) of public enchantment changing to disenchantment, loss of political and economic support, visions of massive professional unemployment, a strong current of anti-science, and perceptions that science may be an instrument for war and ecological rape rather than the increaser of wealth of kingdoms and the bringer of enlightenment. It is a topic that seems to be in rapid transition itself from daring radical disquiet to hackneyed and pious remorse, and this volume provides probably the best instant snapshot of the action, crystallized by the always superb editing of the Ciba Foundation into such a perfect petrification of Oxbridge common room conversation that one can almost hear the passing of the port and cigars. The participants, free from the unemotionality of scientific discourse, are appropriately Messianic in their analyses of what is wrong and what might be done about it, with touches here and there of *schadenfreude* and of spleen. In many ways it is odd to see such social scientists as Shils and Freeman drop their objectivity which would lead them towards consensus and be carried along instead with their peers amongst the natural scientists into very subjective and idiosyncratic statements. Somehow one gets something of the impression of being privy to the conversations of an *intelligentsia* of fruit flies going through a crisis of overbreeding within the confines of a bottle. The poor flies, oblivious of the mechanism of their logistic limitation, fall to complaints and exhortations, *ad hoc* explanations of local breeding behaviour, and the deviant views of the young. In addition

to saying all the things one might at present, the book contains a brilliant little suggestion by Alvin Weinberg for what he calls trans-scientific decisions, a fine analysis by Marcel Roche of the fact that "science does not come easily in Spain", and a fund of very literate and epigrammatic phrases that will be around for some while.

DEREK DE Solla PRICE

Learning to Stop

Inhibition and Learning. Edited by R. A. Boakes and M. S. Halliday. Pp. xv+568. (Academic Press: London, 1972.) £7.25.

As well as trying to teach animals things, psychologists are frequently interested in training them not to do things. Some of the methods for suppressing behaviour are: extinction (withholding of reward), satiation, (lowering the animal's demand for the reward), punishment, rewarding of incompatible behaviour, and distraction by an external stimulus, which may either be novel (Pavlovian external inhibition) or have been paired previously with non-reinforcement (conditioned inhibition). The related questions that arise from this list are, first, whether these different methods of suppression have anything in common; and, second, whether suppression, or "inhibition" of behaviour, is, in any circumstances, a process in its own right, needing to be distinguished from other modifications of behaviour. The twenty chapters in this book, originally papers delivered at a conference in April 1971 at the University of Sussex, are attempts, each in its own way, to answer these questions.

Taken purely as a descriptive term, there is nothing wrong in using inhibition to cover all procedures causing a response decrement. But the tendency to reify inhibition as a process proves to be irresistible: the more so because in its very origin, in Pavlov's theory, it was a hypothetical process in the nervous system, an anti-excitation preventing expression of a learned response. Viewed in such a way, inhibition ceases to be a descriptive term; it becomes

instead a process responsible for some response decrements but not for all. The general idea is that inhibition is not merely "learning to do something else"; nor is it "doing nothing in particular": it is an active restraining of a tendency to respond. All very well, but how is this distinction to be given experimental meaning? The easiest case is where the antagonism is observable, as in the case where the dog fights the experimenter's attempts to move its paw (Konorski). But few cases are so clear. The most influential view at the symposium was probably the Hearst-Rescorla criterion: that inhibition is an effect of stimuli upon the animal, recognizable in two main ways: (1) an inhibitory stimulus will attenuate an animal's response to an excitatory stimulus, (2) learning to respond to an inhibitory stimulus will take longer than to a neutral stimulus. Clear demonstrations of these effects are to be found in the chapters by Hearst and by Wagner and Rescorla. Note that inhibition so defined is well away from the response side: this is very clear, for example, from Hearst's remark that a certain experimental result "could be simply interpreted in terms of a prior conditioning of a 'reduced keypecking' to the tone; no postulation of conditioned inhibition seems necessary".

Such a specific use of the concept is amply guarded against Skinner's old objection to inhibition, that a term so broad as to cover all cases of response decrement is unlikely to be of much value. But one could hardly expect all the contributors to this volume to stick to the Hearst-Rescorla definition. A major contender is what we can call the "aversiveness" criterion. The idea here, going back to early debates about the role of the negative stimulus in discrimination learning, is that an inhibitory stimulus, rather than being something to which the animal has learned not to respond, is actually something unpleasant, to be avoided. Thus Terrace produces evidence that stimuli signalling non-reward are aversive to the pigeon, and cause emotional behaviour. Amsel reviews his theory that non-reward, and stimuli associated with it, produce emotional frustration. The data on this

point seem to be ambiguous, however, for Honig reports a failure to find avoidance of negative stimuli in the pigeon.

Another criterion, related to that of "aversiveness", is that inhibitory stimuli should produce "behavioural contrast" (Terrace; Halliday and Boakes). This is the phenomenon that suppressing responding in the presence of one stimulus can raise response rates in the presence of another (rewarded) stimulus. Not all methods of suppressing responding produce this effect, so it is a useful criterion for classifying them into different groups. "Contrast", under its Pavlovian name of induction, is also considered by Ison. A suggestion which might be relevant to the explanation of contrast is put forward by Richelle, who suggests that after a period in which responding is inhibited, the animal finds it necessary to compensate by energetic responding. If compensation is prevented, as on a schedule that reinforces only low rates of responding, performance can break down entirely; if, on the other hand, the schedule does not explicitly prohibit such responding, it will occur even though not reinforced. Paradoxically, if high rates of response are permitted, the animal is able to separate periods of responding by pauses much longer than those it is capable of making when it is required to pause.

A further criterion, also growing out of the question whether negative stimuli in a discrimination problem are actively inhibitory or merely neutral, is the "inhibitory gradient" criterion. If the animal has learned to respond at a low rate in the presence of a stimulus, will its rate of response in the presence of a "neutral" stimulus be lower or higher? If more responding occurs to the new stimulus, which has no explicit history of conditioning, then it can be concluded that the negative stimulus does something beyond not controlling responding.

None of these definitions includes response suppression by extinction, satiation, or punishment. Brimer, however, wishes to bring these in on the basis of a further criterion: that of "disinhibition". This refers to the re-appearance of a suppressed response caused by presentation of a novel stimulus. The resumption of lever pressing by satiated animals in response to a light or bell is of particular interest (but it is a pity that Drew's investigations of "disinhibition of eating" are not mentioned).

How are these multiple definitions of inhibition to be put together? To take a hypothetical example, how would we assess the claim that inhibition is involved in extinction and in successive discrimination problems, but not in suppression of response rates by delivery

of free food? A correlational approach is indicated in which we define some variable as affecting the animal's capacity to form inhibition, and then see in which kinds of response suppression it is impaired. Here the effects of drugs and brain lesions could prove valuable, as described in the chapters by Warburton, Dickenson, Isaacson, and Douglas. For example, the range of inhibitory procedures that have their effects altered by hippocampal lesions is impressively large. It would, however, be going too far to say that this approach has revealed consistent correlations in its progress to date.

Molnar and Grastyan bring inhibition into the realm of motivational theory. Stimulation of "inhibitory" brain sites brings about withdrawal movements; stimulation of excitatory areas leads to approach. The authors of this paper remind us that motivation in its original sense refers to an urge for locomotion. It is a bit surprising that other authors did not consider the relevance of their work to motivational theory; there is a danger that inhibition will become just a fashionable word for an old concept. The whole problem of "non-behaviour" crops up in motivational theory in the debate about choice and vigour as determinants of behaviour. It has been suggested that a concept of vigour is superfluous because to say that an animal performs a response less vigorously is to say that, if we looked closely enough, we should find incompatible behaviour interfering with that response. Thus, all we need is a theory of choice. The idea of inhibition seems to me to be saying fundamentally, on the contrary, that factors beyond choice are important: a given response can, in some sense, be selected, but not subsequently expressed. The idea is familiar in motivational theory.

Inhibition and motivation are concerned with the "energizing" of underlying tendencies. The basic idea in each case is of an underlying neural connexion, along which mental traffic is speeded up, slowed down, or stopped by excitatory and inhibitory processes. The whole picture is highly suspect, now that connexionism has lost its plausibility. Lashley eliminated connexions from the brain, Skinner from behaviour, and Merleau-Ponty from philosophy. What is left to be "inhibited" or "motivated"? Still, one cannot object to experimenters using a paradigm that has led to such a wealth of interesting data. This stimulating collection of papers will be of particular interest to those who like mixtures of drugs, brain lesions, behaviour and theoretical speculation; just as it will infuriate those who see in big words like "motivation" and "inhibition" a confusing tendency to lump together different things.

M. J. MORGAN

Abacus

The Abacus: its History, its Design. its Possibilities in the Modern World. By Parry Moon. Pp. xii + 179. (Gordon and Breach: New York, London and Paris, January 1972.) £6.20 cloth, £2.05 paper.

THIS book enthusiastically recommends the use of the abacus, gives the available history, discusses various forms and describes a considerable range of application. There is a rather frank emphasis on the use of multiplication tables and, since elementary arithmetic is not usually a source of joy in childhood, the reader may be hesitant in his acceptance. But no other computing device rewards arithmetical ingenuity and knowledge as well and even the necessity of multiplication tables justifies the use of Napier rods so that one can happily manipulate two devices rather than one. One can therefore learn to appreciate the virtues of this solid state computer so reliable that the only errors involved in its use are due to the operator. No wonder the clever Japanese export those complex electronic gadgets and retain the soroban for themselves.

The abacus is widely used in the Orient in two biquinary forms and there is a Russian mine bead per rod set up. Before inexpensive paper was available, Western arithmetic was performed either on the abacus or on its close relatives the counting board or cloth. Present-day rotary calculators and their electronic counterparts use the decimal or base ten number representation. If one wants to handle numbers in a representation with another base such as eight, sixteen or sixty, the author points out that the combination of appropriate abacus and Napier rods is the most inexpensive way of obtaining computational assistance. Some ingenuity is required to make such an approach practical but this is probably considered a virtue. Decimal forms of the abacus are available as toys which can be quite educational if guidance is provided.

The author discusses arithmetic procedures which assist in the use of the abacus. One can vary not only the number base for an abacus but also the representation of the digits in each place. For example, in a decimal abacus, the digits can be represented either by the use of unit markers or by markers with values one and five or by markers with values one, two or four. The author claims that place representations, in which the total value of the markers exceed the minimum required, permit a significant reduction in the amount of manipulation but it is not clear to me that his discussion takes into account the full effect of redundancy. The use of various present day forms of the decimal abacus is given in detail.

The effective use of the abacus has the fascination of a complex skill with a multitude of variations so that the *aficionado* has many avenues to explore. But there is no written record of the procedure and there are so many competing devices, whose initial barrier is not skill but cost. Despite these limitations on the use of the abacus itself, the book is informative and enjoyable.

F. J. MURRAY

All About Climate

Foundations of Climatology: An Introduction to Physical, Dynamic, Synoptic and Geographical Climatology. By E. T. Stringer. Pp. xiii+586. (W. H. Freeman: San Francisco and Reading, April 1972.) \$17.50; £6.30.

Techniques of Climatology. By E. T. Stringer. Pp. xiii+539. (W. H. Freeman: San Francisco and Reading, April 1972.) \$17.50; £6.30.

AFTER a famine of several decades, British climatologists are enjoying the fruits of the long labours of three of their number. Close on the heels of Professors Crowe and Lamb, Dr E. T. Stringer of Birmingham University presents us with the results of sixteen years of work in two handsomely produced volumes. He aims to provide a rigorous, comprehensive and yet easily understood coverage of climatology and the present thousand or so pages are meant for university students, teachers of physics, mathematics and geography and entrants to state meteorological services.

Foundations has six chapters following some notes on the history of climatology and a few brief thoughts on approaches to its study. The first chapter deals mainly with pressure distribution and the mechanics of pressure change, ranging from localized pressure jumps to global atmospheric tides. This is followed by a chapter on atmospheric properties and processes restricted mainly to coverage of energy and vertical motion. Chapter 3 is a laudable attempt to introduce the reader at an early stage to the insights into atmospheric behaviour which accrue from an appreciation of turbulence at both the micro- and macro-scales. The longest chapter in the volume deals with the general circulation, considering in particular the circumpolar vortex, long waves, jet streams, index cycles and regional circulations, particularly those in polar and tropical areas. The chapter concludes with a relatively brief section on explanation and some thoughts on the implications of general circulation research for studies of climatic change. The last two chapters of this volume are concerned with familiar thermodynamics and related topics and synoptic climatology.

Techniques has three parts: one on

basic techniques; one on their application; and a final chapter on "Geographical Climatology". In part I, three chapters deal with observing techniques, interpretation of the observations (mainly statistical analysis of temporal and spatial series) and climatological models—both experimental and mathematical. Part II comprises four chapters on radiation, temperature, clouds, and visual climate and optical climatology.

The principal impact of these books lies in the sheer amount of information they contain. The reader is explicitly exposed to virtually all the significant meteorological literature in published journals and reports in the English language over the last thirty, if not more, years. The thousands of references involved in this remarkable feat are accommodated in hundreds of lengthy footnotes which frequently occupy over five pages at the end of each chapter despite the use of many abbreviations and a small type-face. In addition, each chapter has a substantial appendix containing mathematical details of points made in the text. Whether the books are considered from a meteorological or a geographical viewpoint, Dr Stringer's awareness and meticulous documentation of this tremendous amount of material put his work into a class of its own.

But the difficulties of handling all this information are reflected in three closely related ways. First, some readers would perhaps query the organization of the chapters and their contents. To take two of many possible examples, *Foundations* chapter 2 ("Properties and Processes") could well be linked with the chapters on turbulence and thermodynamics. Yet it could also be argued that if, as Dr Stringer states, turbulence is a technique (*Foundations* page 174), it should be dealt with in the companion volume. Again, the section on synoptic analytical methods (chapter 6) could arguably be included in *Techniques*. Secondly, and perhaps more important, there is a lack of balance in the treatment. One is left with the impression that everything has been included, with too little discrimination and synthesis. Thus, in considering the general circulation, jet streams are dealt with in great detail and there is ample coverage of tropical circulations at and below even the synoptic scale; yet only just over one page is devoted explicitly to the Rossby waves. Thirdly, the style and method of both books are those of a review work requiring a high level of expertise on the part of both author and reader. On his own admission, Dr Stringer took more than a decade to achieve his present competence and one doubts the ability of even the keenest student to approach a match of it. The demands made of the reader are typified

by the treatment of Smagorinsky's general circulation model. The reader is confronted with a half-page appendix giving only the briefest outline of the relevant equations. They will have real meaning only to the already cognizant. Further, confidence in the accuracy of the appendices is somewhat undermined by the confusion evident in the supposedly simplified mathematical statement that the latitude-longitude coordinate system on the rotating Earth is accelerating. Despite the above criticisms, one cannot but admire the scope of these volumes. It is a pity that their encyclopaedic nature is hindered somewhat by numerous errors in the indexes, but as a testimony to diligence in seeking an understanding of the "working substance" of climatology they stand unparalleled.

B. W. ATKINSON

Hormone Control

The Sex Steroids: Molecular Mechanisms. Edited by K. W. McKerns. Pp. ix+454. (Appleton-Century Crofts: New York, 1971.) \$27.50.

THIS book is the third volume in the Biochemical Endocrinology Series edited by Dr McKerns—"a continuing series of monographs emphasizing how the synthesis and secretion of hormones are controlled, how the pathways of hormone biosynthesis are regulated and how the cellular biology of the target organs are stimulated or inhibited by hormones". The prominent display of the title *The Sex Steroids* on the dust jacket and spine of this book tends to mislead because, as the full title indicates, it deals only with the last topic claimed for this series of monographs, actions at the molecular level; and why call them sex steroids, when their rôle goes far beyond sex?

There are fifteen chapters, only one of which deals with the actions of androgens, although much has been learned about these hormones. The majority of those American biochemists who have contributed to this field of investigation appear on the list of contributors. The book stems from papers given at a symposium (undated) in Florida, and one wonders if investigators from outside the United States were excluded on grounds of economy.

This is not a book for beginners. Almost without exception, each author plunges at once into a detailed account of his experiments on steroid receptors in one tissue or another, with the usual figures showing radioactivity plotted against fraction numbers. It is, however, a pleasure to read the views of some of the earlier workers in this field, like Clara Szego (chapter 1), who remember that the ultimate explanation must encompass all observations. How right she is—"With notable exceptions, our preoccupation with this pheno-

menon (information sequestered in the nucleus) over the past decade has tended to obscure our view of events in the cytoplasm."

The chapters vary considerably in interest and clarity, and while it is probably invidious to pick out individual authors, one cannot help thinking that the work described by Dr B. O'Malley is bringing us very close to the truth about how steroids act. Of course, those of us who are concerned, hear or read the story, slightly altered, so often, we are liable to become very blasé about what is new. Kidson's account of specific steroid-nucleic acid interactions is refreshingly different, even if he has to speculate rather hard about possible biological significance. The comments of Dannenberg (1963) published in Germany on similarities between the molecular geometry of steroids and projections of base pairs occurring in the DNA helix are not, however, mentioned. Science should be international.

The accounts of work presented are enlivened by very full discussions. Because Dr McKerns collected most of the American authorities in the field at this symposium, these discussions are very well informed.

Although it is not always easy reading, and the index, on four pages, is skimpy, this book is recommended to those who would try to keep abreast of developments in our understanding of the actions of steroids at the molecular level.

J. K. GRANT

Steroid Chemistry

Organic Reactions in Steroid Chemistry. Edited by John Fried and John A. Edwards. Vol. 1. Pp. xv+510. £12.25. Vol. 2. Pp. xi+464. £11.30. (Van Nostrand Reinhold: New York and London, April 1972.)

THESE two volumes, edited by chemists from the Syntex Laboratories, present a very welcome collection of reviews covering in depth fifteen selected topics in steroid chemistry. Each has been written by a chemist with expert knowledge of his field. Even steroid chemists of long experience will discover much valuable material which has lain buried in the literature, and there can be few chemists concerned with organic synthesis who would not find stimulation from browsing through this impressive survey of a wide variety of reactions.

Djerassi's *Steroid Reactions* (Holden-Day, 1963) gave us a most valuable collection of lists of reactions, with references. The present volumes, inspired from the same source, develop the same idea by providing also a highly informative text. The reactions, their ranges of applications, and where appropriate their stereochemistry and

mechanisms, are all covered in considerable detail. A special feature, to be strongly applauded, is the inclusion of carefully selected experimental instructions for most of the important reactions discussed, which often make reference to the original literature unnecessary. Each chapter is, however, well supported by references, which total nearly 2,800, though doubtless with some duplication between chapters: literature coverage extends for the most part into the early part of the year 1970. There is a useful "Index of Experimental Procedures" as well as a comprehensive subject index.

The section on reductions of steroid ketones by hydride donors contains detailed tabulation of stereochemical results at all the steroid positions, and there is an interesting account of the history and present state of controversy over the mechanism of reduction. A wide spectrum of compounds and techniques is covered in the discussion of methods for introducing deuterium, and the section on fluoridation includes descriptions of the use of several potent reagents developed only recently. The chapter on alkylation procedures omits a wide variety of indirect alkylation procedures, for example the Wittig, Mannich, and Vilsmeier reactions—I mention this omission here not as a criticism, but rather as a hopeful suggestion for any future third volume.

Although the price of these two volumes will put them beyond the reach of most individual chemists, their contents represent excellent value wherever chemists are engaged in the synthesis of steroids or comparable organic molecules.

D. N. KIRK

The Working Brain

Brain Mechanisms and Mind. By Keith Oatley. Pp. 216. (Thames and Hudson: London, April 1972.) £2.25 cloth; £1.25 paper.

GLOSSY books about science are not a new phenomenon, but in few areas can such popular writing be so close to current research as in sensory psychology and neurophysiology. By happy coincidence a popular thirst for reliable information about the workings of the mind is complemented by a body of experimental evidence that is nowhere near as inscrutable and apparently obscure as, say, quantum mechanics and particle physics. This has led to such excellent books as Wooldridge's *The Machinery of the Brain* and Gregory's *Eye and Brain*. Now Keith Oatley's new book follows in the same tradition.

Certainly superlative in its glossiness, this short book in the World of Science Library series has much more to commend it than glamour alone, although Thames and Hudson have gone to town on the illustrations, which actually

occupy about half the printed area of the book. Nearly all the pictures are very pretty and most of them are more than mere embellishment. Sadly, the cover alone suffers from being neither excellent in design, nor, apparently, in any way relevant to the book's content.

Keith Oatley's own research interests embrace the somewhat eccentric combination of a study of the regulation of thirst and work on visual perception. He has made a masterful attempt to build his book around these two main themes without making the mixture seem absurd.

He starts with a description of nerve cells and quite a full account of how nerve impulses are conducted. Then comes a discussion of the localization of functions within the brain, which concentrates on the rise of phrenology in the nineteenth century and on Wilder Penfield's observations while stimulating the human brain during neurosurgery.

The section on visual perception (naturally particularly rich in illustrations) principally covers Gestalt theory, visual illusions and the perception of distance. Thirst and hunger follow hard on vision, with the obvious analogy between mechanical servo control systems and the living control systems that regulate eating, drinking and sex. There is a short discussion of learning and memory and then an oddly incongruous account of Chomsky's linguistic theory.

Perhaps the single unifying idea in the book is its constant comparison between mental processes and the kind of logical operations that computers can perform. Obviously there are grounds for comparison between real and artificial brains but in sheer quantitative terms genuine grey matter is incomparable. How can any present day electronic device compare with the brain's system of ten thousand million components each with up to ten thousand inputs? The comparison reminds us of just how bad digital computers are at some things (like pattern recognition) and how good they are at others (like numerical computation).

So the book is catholic in its subject matter and but for a few small errors is both authoritative and accurate. It is a useful introduction to the methods and more exciting results of modern brain research and is certainly stimulating reading for the layman. Unfortunately, Oatley has succumbed to the massive temptation to popularize brain science by exploiting all the trendiest topics: drug culture, brain transplants, artificial brains and science fiction all creep in somewhere. Perhaps this approach may be justified if, as in this case, it brings an exciting book to a wider audience but it is regrettable that pseudo-science must be used to sugar the pill of scientific reality.

COLIN BLAKEMORE

INSTRUMENT REVIEW

Measuring Gas in Blood

Model 165 pH/Blood Gas System
(Corning-EEL Scientific Instruments,
about £2,150).

THE Corning-EEL pH/blood gas system is a semi-automated system for routine estimations of the P_{O_2} , P_{CO_2} and pH of blood. From these values, it calculates actual bicarbonate concentration, total CO_2 and base excess using a built-in electronic calculator. The instrument consists essentially of a Clarke-type oxygen electrode, a Severinghaus-type carbon dioxide electrode and a glass capillary pH electrode with calomel reference electrode. There is no KCl junction between the glass and reference pH electrodes; instead, the reference electrode is in a KCl jacket covered by a semi-permeable membrane. All these are in constant contact with a single sample chamber, the principal cuvette being situated at the end of the glass capillary, through which all samples must pass. The principal cuvette is made of stainless steel and aluminium with a non-thrombogenic coating, and takes a sample as small as 125 μ l. The temperature of the cuvette is controlled electronically to the predetermined temperature within the range 36.0° C to 38.0° C, thus eliminating the necessity of a water bath. The time for a sample to reach its equilibrium temperature is approximately 30 s. There is a continuous electronic digital readout display through a special non-glare panel, and any function can be read directly by pressing the appropriate button when the sample is in the chamber. pH values range from 0.000 to 14.000 in steps of 0.001 pH units, P_{CO_2} values from 10 to 260 mm Hg in steps of 0.1 or from 100 to 2,600 mm Hg in steps of 1 and P_{O_2} values from 0.0 to 1,600 mm Hg in steps of 0.1. A timer on the back panel of the instrument can be set so that the readout stays stable thereafter. There is also an analogue output socket.

The oxygen and carbon dioxide electrodes are calibrated using two gases which are connected to two humidifiers at the side of the machine. The d.c. voltage bias control for the carbon dioxide electrode must be set first with a screw at the back of the instrument. The gases are fed through the sample chamber at the press of two buttons; one for the zero oxygen/high carbon

dioxide gas (10% CO_2 ; 90% N_2) which should only need setting once or twice a day, and the other for the low carbon dioxide/high oxygen gas (5% CO_2 ; 19% O_2 ; 76% N_2) which should be set before every estimation. Gas flows can be adjusted with knobs on the humidifiers, and an automatic shut-off valve operates if gases are accidentally left on, so the gas cylinders must be equipped with pressure heads. The pH electrode is calibrated by inserting a low buffer (pH=6.841) and a high buffer (pH=7.383) into the sample inlet. There are two calibrating knobs for each electrode; one for adjusting the lower calibration point and one for adjusting the higher calibration point. The sample, preferably 200 μ l., is then fed into the sample chamber from a syringe or a capillary tube. If only 125 μ l. of sample is available, it will only fill the glass capillary electrode, so the pH value should be read first. The sample can then be pushed on further into the principal cuvette for P_{O_2} and P_{CO_2} readings. A warning light appears if either the oxygen or carbon dioxide membranes are defective. The other variables are calculated to 0.1 mmol l.⁻¹, or mEq l.⁻¹ in the case of base excess, using the P_{CO_2} and pH values found, the Henderson-Hasselbalch equation and the haemoglobin concentration, which must be fed in manually by setting a dial. The cuvette is cleaned by pressing a button to release wash solution and another button which produces suction, with the operator deciding when, and for how long, cleaning is required.

On thorough testing, each electrode was found to have remarkable stability and quick response times. The latter were within the manufacturers' specifications of 15 s, 30 s, and 45 s for the pH, oxygen and carbon dioxide electrodes respectively during calibration, although it was found necessary to wait approximately 2 min for the oxygen and carbon dioxide electrodes to stabilize with blood. The pH electrode had a response time of approximately 30 s with blood. The oxygen electrode was found to be reasonably linear, the maximum error being about 4%. The linearity of the carbon dioxide electrode using a range of CO_2 gas mixtures was poor, the error obtained being between 0 and 10%. The manufacturers were consulted and they found, on in-

vestigation, that the electrode was linear with gas-equilibrated water. More work should be carried out concerning this point. Maximum errors for P_{O_2} and P_{CO_2} on tonometered blood were approximately 3%. There were errors of 1 to 2% on reproducibility with both gases and blood for the oxygen and carbon dioxide electrodes; the reproducibility of pH measurement was within 0.003 pH units. These figures conform to the manufacturers' specifications. The pH electrode was found to drift considerably if the sample was left in for more than 2 to 3 min. The temperature inside the cuvette was found to change by 0.2° C in a period of 3 h, which is outside the manufacturers' specifications of 0.01° C.

In summary, the whole system is very easy to use and maintain. Its semi-automated nature makes it ideal for routine work performed by different technicians; the chief drawback is that it is necessary to remember to wash out the system and fill it with buffer, which is particularly important for the survival of the glass capillary electrode. Apart from this, the fact that all samples must pass through the capillary may be an advantage in that it may help to prolong the life and stability of the electrode. The pH reference electrode does not give such stable results as one incorporating a KCl junction, but the results were quite adequate for the kind of work involved. Maintenance is made easy by the elimination of the KCl junction and a water bath, and also because all the sample tubing is situated outside the chief part of the instrument comprising the electronics. The mechanism for temperature control is claimed to be much more accurate than a water bath, and the metal cuvette helps temperature stability.

The principal disadvantages in the design are, first, that the actual output (mV) of each electrode cannot be obtained, so there is no guide to the fall-off of electrode sensitivity; and, second, the positioning of the CO_2 electronic d.c. bias control is awkward. Also the three calculated variables should not in my opinion be used, because patients may have an abnormal pK_1 for carbonic acid (see Trenchard, Noble and Guz, *Clin. Sci.*, **32**, 182; 1967). These small disadvantages are, however, compensated for by the comparatively low price.

WENDY ELSE

CORRESPONDENCE

British Astronomy

SIR,—As a result of the vagaries in the transatlantic mail, I have only just seen the remarkable letter by Geoff Burbidge on British optical astronomy (*Nature*, **239**, 117; 1972). I should like to endorse everything said in that letter; at last somebody has had the courage to publish the truth on the lamentable state of this subject in Britain. The situation has been fairly obvious for years but too many of us have lacked the guts to say so for fear of upsetting an apple cart which has in fact been stuck in a mire of shortsightedness and national self-satisfaction for the last half century. British optical astronomers must judge and be judged on an international scale. On this scale, Britain has undoubtedly been getting a very poor return for money invested in the field and no excuses or rationalizations (for example, poor weather) can alter the fact.

Clearly this situation cannot be allowed to continue. One solution would be to get out of optical astronomy altogether and to close down existing observatories. In my opinion this would be wrong, at least until alternatives have been tried. In the foreseeable future astrophysics will be one of the most exciting and productive areas of physical research. It is also probably true that optical astronomy will, for some time, be potentially able to deliver more information per unit investment in time and equipment than astronomy in other wavelength bands. (For example, the only reliable distance indicator for extragalactic objects is furnished by optical astronomy.) A viable optical astronomy programme is thus important if not vital for a healthy development in astrophysics. This point has been recognized by the Americans in setting up National Observatories at Kitt Peak and Cerro Tololo, by the Russians in building the 240 inch telescope at a site in the Caucasus Mountains and by various European countries in establishing the European Southern Observatory (ESO) in addition to their various national schemes. This point of view has also been accepted by the SRC which has continued to provide adequate funds for optical astronomy. The main barrier to progress lies in the British astronomical establishment, both in its organization and in its membership. Only if both are changed drastically can British optical astronomy hope to make the grade internationally. Only then will Burbidge's catalogue of major mistakes come to an end.

Given that the sickness has at last

been recognized, what is required for a cure? First of all, a new National Centre must be set up along the lines recommended by the NHRC and should be responsible for building and running all overseas optical (and infrared) installations. The old observatories just will not do since their habits of inertia seem too deeply ingrained to change; they should be pruned, preferably sharply. (Some of your readers will no doubt be aware that the Germans have, in similar circumstances, adopted precisely such a policy in setting up the Max Planck Institut für Astronomie at Heidelberg.)

To ensure rapid progress, the new Centre must be staffed with demonstrably successful astronomers even if this inevitably means recruiting from overseas, at least initially. Decent observing facilities of proven design must be set up as soon as possible at a site of established quality. This means appointing a project manager with full responsibility for the scheme and an end to the practice of eternal consultations with everybody. Allocation of observing time and funding of research proposals should be much more competitive and preferably subject to review by international experts. Results should be the only criterion, not accent, origin or some long standing tradition. Above all, good leadership is required if a revival in optical astronomy is to take place. In this respect the recent return to England of Geoff and Margaret Burbidge augurs well. Together they constitute one of the most successful research teams in astronomy today. Given the support both of British astronomers and of the SRC they stand an excellent chance of revitalizing the British astronomical scene. Having worked with them in the US I should, however, emphasize that their overriding interest is in obtaining astronomical results and that if continued attempts are made to frustrate their efforts (particularly in the name of the "British way of doing things") I, for one, doubt that they will stay for long.

The time has obviously come for the British to decide whether they are serious about optical astronomy and, if so, to insist on a new, more cooperative and more outward looking way of proceeding. If not, they might as well get out of the field altogether and save themselves a considerable amount of money and ridicule.

Yours faithfully,

P. A. STRITTMATTER

Steward Observatory,
University of Arizona,
Tucson, Arizona 85721

Katchalsky

SIR,—The creation of an international Katchalsky fellowship has been criticized by some of your correspondents who state that this would provide moral support for the Israeli government (*Nature*, **238**, 236; 1972). It is not our intention to discuss politics in *Nature* but we feel obliged to reply on behalf of the memory of the scientist and humanist Aharon Katchalsky.

It is evident that the choice of any country on Earth for the location of an international institution has some political implication. But there is indeed no government that could not be criticized and condemned by some scientists for some aspects of its politics: the United States for the Vietnam war, the Soviet Union for the invasion in Czechoslovakia, France for the Algerian war, and so on . . . If military affairs were sufficient to prevent the establishment of international scientific institutions in a country, the only place of choice would be another planet. Once it is recognized that there is necessity for science on Earth, political considerations should never be a reason for exclusion of the scientists of any country from the international scientific community. One of the results of such a discriminatory and unscientific attitude can be seen in the manner your two correspondents treat historical information. For instance, they try to link Katchalsky as a former leader of the Hagannah to Deir Yassin and other Arab-Israeli hostilities. In reality, Deir Yassin was the act of some men of the Irgoun group. The Hagannah fought this group and the Irgoun terrorists responsible for Deir Yassin were prosecuted.

The efforts of many scientists who, like Katchalsky, stand up for humanity and against wars should not be undermined by distortions and the request to boycott the scientists of a particular nation.

Yours faithfully,

GERARD WEISBUCH

Département de Physique,
UER,
Marseilles Luminy

EBERHARD NEUMANN

Max Planck Institut
für biophysikalische Chemie,
Göttingen

Science and Politics

SIR,—I wish to reply to the letter of Cohen *et al.* (*Nature*, **237**, 469; 1972) regarding NATO sponsorship of an Advanced Study Institute on proteins of

the nervous system. This is not the first attack on the NATO Science Programme, and it displays the same reluctance as others to consider the relevant and quite readily available facts. The authors of the letter raise three questions and then make a number of points of debatable relevance. Perhaps direct answers to these questions would be helpful to your readers.

What are the motivations and purposes of the sponsors? The NATO Science Programme was established in 1958 to strengthen the competence and capabilities of the scientific communities of the member countries. This was (and is) believed to contribute to the security of these countries as do strong and viable economies, orderly political institutions, cultural ties and even military capabilities. The programme is designed and guided by a Science Committee composed of senior scientific advisers from each country; the consensus of their individual viewpoints decides the programme. Their goal is clearly the healthy progress of science through provision of opportunities for education, research and communication. Since 1958 approximately 10,000 NATO Fellows have spent up to a year or more in the laboratories of foreign colleagues. Over 500 research grants have brought together techniques and samples, theoreticians and experimentalists, measurements and their interpreters, in fields ranging from aeronomy to zoology. Some 600 Advanced Study Institutes (ASIs) have been sponsored, permitting about 40,000 advanced students to take advantage of coherently structured presentations by distinguished groups of international experts.

Is free scientific communication a permitted consequence of the meeting? All of the above activities are carried out with the complete intellectual freedom and communication which is the hallmark of the good scientist. A majority of the ASIs result in published proceedings, many of them considered benchmarks in authoritative treatment of their subjects. Anyone who has taken part in an ASI will confirm that informal, uninhibited communication, during and after the meeting, is strongly encouraged and is a key element in the high reputation of this series (for example, compare Giorgi, *Nature*, **238**, 57; 1972).

Those interested in directing an ASI choose a subject in their own fields of interest and make application to the NATO Secretariat. An advisory panel, composed of distinguished scientists, serving as individual experts rather than governmental representatives, selects the successful applications on the basis of scientific importance and timeliness of subject, and quality of lecturers. The director then selects the participants. It is thus the individual ASI director who has complete control over the choice of

subject, selection of participants, conduct of the ASI and the ultimate publication of its proceedings. Over the years a consistent 10%–20% of the participants have come from non-NATO countries; nationals of almost seventy countries, including Eastern Europe and the USSR have taken part in ASIs. The records show no participation from the Chinese People's Republic or North Vietnam, but this has also been noticeably lacking in other scientific meetings held in Western Europe and North America.

Are the deliberations of the meeting of use for military purposes? Space does not permit a listing of all the ASIs sponsored by NATO; such a list is available upon request. The first six titles in the published 1972 programme may be given as examples: "Magnetic Resonance in Inorganic Chemistry"; "Feldspars"; "Data Bases: Theory and Implementation"; "High Energy Astrophysics"; "Molecular Dynamics in Simple Liquids"; "Modular Functions".

To the extent that the results of these meetings, or any others, or any published research, anywhere, are of use for military purposes, they will quite possibly be so used by anyone who follows such matters for such purposes. But the implication that the fifteen countries in NATO have conspired to spend some £20 million over the last fourteen years for the purpose of reaping direct military benefits from a few deliberately designed conferences is really far-fetched.

Yours faithfully,

EUGENE G. KOVACH

North Atlantic Treaty Organization,
1110 Brussels

Scientific Community

SIR,—The proposal presented below represents a possible way of remedying three major problems (in my view) of contemporary science—a disintegrating environment within science itself, the misuse of the results of scientific studies and overwhelming problems of communication both within science and between scientists and the general society. The proposal deals specifically mainly with the first problem, but might help with the other two as well.

Most people who become scientists are attracted to the field for a varying mixture of two rather idealistic motivations: the creative joy of it and a feeling of potential contribution to the welfare of humanity. Instead, the contemporary atmosphere is often far different—that of a collection of entrepreneurs struggling for personal power and self-glorification with great determination but little joy, creative or otherwise. The drive to be first to publish is, of course, the major symptom of this atmosphere.

Secrecy, jealousy, exploitation and despair are the costs.

To try to improve the situation, I suggest that scientists with a common research interest form a true intellectual community around that interest. Progress from such a research group would be reported about twice a year in a book which would be derived from a meeting of the group. The book would present data, conclusions and discussion for each sub-area of current research in the form of a collection of papers, but individual contributions would not be identified in the published work. Everyone who contributed would be listed as an author (in alphabetical order) of the book. Each sub-area would have an editor (or editors) who would differ from year to year and be chosen by election. The role of the editor would be to introduce and summarize an area of research—not to decide what is or is not published. People unable to attend could send in their contributions. The book could be published rapidly (perhaps by a photographic printing process) since editorial work should be minimal. The meetings would be advertised well in advance and would be open to anyone; however, each group meeting should be typically rather small because it would deal with an area of specialization (the Temperate Bacteriophage Meeting at Cold Spring Harbor is one example of such a possible group meeting). Alternative proposals are discussed below.

I feel that the procedure outlined above would encourage communication, cooperation and the exchange of ideas among members of the group, since emphasis would be placed on advancing the field as a whole and not on individual contributions. We might hope to regard the communal nature of scientific progress which has already existed as a desirable and important feature rather than something which should be minimized in an effort to emphasize personal contribution. Furthermore, communication with other fields would be greatly improved, since progress in one area of research would be presented, summarized and discussed in a single place, rather than scattered through many journals at many different times. (Of course, communication of results and ideas would not be limited to the annual meetings—only formal publication.) Finally, an association of scientists truly dedicated to the value of science as a creative undertaking and benefit to humanity would be more effective and more highly motivated in pointing out potential values and misuses of their research to the general society.

The major potential disadvantage I can see is that granting agencies might complain that past contributions were not extensively documented. However, since the grant is presumably awarded

mainly for the proposal and not the past, this is not a severe problem. Competence in the area could be accomplished by reference to the applicant's work in the group publication (for example, *RNA Polymerase*, 5, 65). Perhaps a less competitive environment would also encourage scientists to worry more about the tendency to a concentration of an increasing fraction of the financial resources for science under the direct control of a few people. University promotion committees would also have to be familiar with the system.

Three alternatives follow, which would in my view be less effective. One of the possibilities is for a group meeting as above, but with individual papers signed as in a typical current symposium.

The race into print would be eliminated, but personal contributions would be documented. My third suggestion is that certain issues (perhaps two per year) of standard journals should be devoted to a particular research interest. This fact would be advertised well in advance, papers would be collected and published together with an introduction and summary (signed) by members of the editorial board of the journal. This should improve communication with other fields and might lessen competition without the technical problems of an organized meeting and book publication. However, the research group would retain less control over its summarizers and the general publishing policy. Yet another possibility is an informal agreement that people would actively arrange with others working along similar lines to publish simultaneously or jointly and to try to minimize duplication. (This is, of course, sometimes done at present.)

Yours faithfully,

HARRISON ECHOLS

Department of Molecular Biology,
University of California,
Berkeley, California 94720

Science Libraries

SIR,—Sandison (*Nature*, 234, 368; 1971) has cast doubt on the accuracy of Brookes' (*Nature*, 232, 458; 1971) model for optimizing a library of scientific periodicals on the grounds that the members of the Bradford set are not constant and that rank within the Bradford set changes with time.

base year are taken into account in the optimization process.

With 100% target percentage all error in the model is due to papers in newly contributing periodicals. The percentage error due to this cause is approximately the same all through the target range of interest. This allows the calculation of an expected percentage of papers in the library in 1940 allowing for periodicals

Table 1 Percentage of Relevant Papers in the Library in 1940 for Various Target Percentages Set by the Bradford Cut in Various Base Years

Base year	Percentage papers in library in 1940 for target % x			Expected % papers in library in 1940 taking into account new contributing periodicals for target % x		No. of contributing periodicals in base year
	$x=50$	$x=75$	$x=100$	$x=50$	$x=75$	
1925	15	25	38	19	29	39
1930	29	44	61	30	46	96
1935	40	57	77	38	58	190
1925*	26	44	59	30	44	166
1930*	33	54	77	39	58	284
1935*	40	72	91	46	68	471

* Optimization process based on cumulated experience to base year by using all papers published up to and including base year for ranking periodicals.

An estimate of these errors can be obtained from Needhams' bibliography (*Biochemistry and Morphogenesis*, Cambridge University Press, 1942) which can be considered an approximation to a complete set of papers relating to the biochemistry of morphogenesis up to 1940. This bibliography is appended to a comprehensive review of the subject and contains 6,668 references to papers in periodicals. It conforms with Bradford's law and the negative exponential distribution of papers with time.

If a librarian has optimized his library by Brookes' Bradford cut in 1925, 1930 and 1935 with the object of providing 50%, 75% or 100% of papers, the actual percentage that would have been in the library in 1940 is shown in Table 1. It also shows the percentage in the library when all papers up to and including the

that begin contributing after optimization (Table 1).

The difference between the expected percentage and the actual percentage represents error due to change in the ranking of periodicals. If the ten most productive periodicals for the years 1925, 1930, 1935 and 1940 are listed each title appears on average 1.6 times. Despite this change in ranking, the error due to this change is small by comparison with error due to new contributing periodicals. For an expanding literature such as the one studied, review of the Bradford set must be more frequent than every five years if Brookes' model is to yield acceptable predictions.

Yours faithfully,

I. A. DOUGLAS

Newcastle Public Library,
Newcastle, New South Wales

Obituary

Dr P. H. Leslie

THE death, in June of this year at the age of 72, of Patrick Holt Leslie has deprived the world of ecological science of one of its most brilliant bi-mathematicians, one to be ranked with Raymond Pearl and Alfred Lotka. Educated at Westminster School and Christ Church, Oxford, he obtained an honours degree in physiology in 1921, but was later prevented by a very serious illness of the lung from completing a medical course. After several

years of research in bacteriology at the School of Pathology in Oxford, he began an association with the Bureau of Animal Population (under Charles Elton as Director) that continued from 1935 to his retirement in 1967. He became a Senior Research Officer there and his activities fell into two closely-related spheres: advisory work on statistical matters, and his own research upon demographic methods and problems. Leslie (known to his family and a large circle of friends as "George") was compelled by his earlier

ill-health to live a quiet and somewhat secluded life. His habit was to visit the laboratory in the morning to advise and read, but to spend the rest of his time working at home. Thus, like Darwin and unlike many in the academic hurly-burly of today, he had uninterrupted opportunities to think, analyse and calculate. Through these years he was greatly helped by the devoted care of his wife. It is to be noted with some astonishment that Leslie had no formal training in advanced mathematics, and his extra-



ordinary talent for original mathematical approaches to ecological problems became apparent unusually late, after the age of 35. He came to be held in high respect by more professionally trained experts in the same field. Hundreds of research workers at the bureau and elsewhere are in his debt for shrewd and patient advice, often also for much careful examination and computation of their figures. His tall figure, quiet manner, friendliness to people of all ages, whimsical humour, and incisive intellectual powers became, so to speak, an institution within the research institute. On matters of scientific integrity he was adamant, and many young and some older research workers gained their first statistical training from him. But the advisory aspect of his activities was also important for the development of Leslie's own ideas; for he

attained a clear picture of the kind of assumptions that ecologists deal with, the long and tedious and usually incomplete nature of field studies on animal populations, and their sampling problems. Thus his own ideas always started from sound ecological premises.

Leslie's first original work on populations was the calculation of the intrinsic or natural rate of increase of a wild animal, using data for voles, *Microtus agrestis*, bred in the laboratory by R. M. Ranson. This calculation employed age-specific rates for births and deaths. Such an estimate had previously been made only for man, those for animals being fallacious guesses based upon reproductive capacity alone. Later he calculated such rates also for the brown rat, the Orkney vole and flour-beetles. The methods were quite novel for animal ecology. He went on to much more complex theoretical studies of population growth and structure (including age-distribution and generation time) and of the interactions between species. His paper "On the use of matrices in certain population mathematics" (1945) is a classical one. During the Second World War he used his skills in the bureau's team research upon the control of harmful rodent populations. In his later years he took up two chief subjects. First, the statistical basis of marking and recapturing live-trapped small rodents (but applicable much more widely) to yield population data, using the field operations of Dennis and Helen Chitty on voles. Secondly, he began a long and brilliant series of studies upon the application of stochastic equations, as compared with the more simplified deterministic types, to the elucidation of predator-prey and inter-species relations. Much of this

was made possible by the programming help of J. C. Gower on the Rothamsted computer. From 1948 onwards, Leslie formed an increasingly close association with Thomas Park of the Department of Zoology in the University of Chicago, who had for many years engaged in laboratory experiments upon competition between two species of flour-beetles (*Tribolium*). Gradually he was able to develop equations that simulated and illuminated the very complex results of some of these experiments. His whole self-made mathematical career, with its comet-like character, must have done much to make people realize the formidable complexities inherent in population ecology, and has provided valuable new tools for use in them. He gained his Oxford DSc on the published results of his various researches.

Reports and Publications

not included in the Monthly Books Supplement

Other Countries

Fisheries Research Board of Canada. Technical Report No. 318: Distribution, Relative Abundance and Growth of Larval Herring *Clupea harengus* L. in the Southern Gulf of St. Lawrence. By S. N. Messieh and A. C. Kohler. Pp. 31. (St. Andrews, NB: Fisheries Research Board of Canada, Biological Station, 1972.) [229]

Geological Survey of Canada. Miscellaneous Report No. 17: Laboratories, Scientific and Technical Services, Ottawa. By Peter Harker. Pp. 42. Miscellaneous Report No. 18: The Geological Survey of Canada. Into the Seventies—the Fourteenth Decade. By S. C. Robinson. Pp. 26. Paper 71-9: Relationships of Structural Lineaments and Mineral Occurrences in the Abitibi Area of the Canadian Shield. By Jan Kutina and Andrea Fabbri. Pp. viii+36. \$2. (Ottawa: Information Canada, 1972.) [229]

US Department of the Interior: Geological Survey Water-Supply Paper 1939-C: Electrical-Analog Analysis of the Hydrologic System, Tucson Basin, South-eastern Arizona. By T. W. Anderson. Pp. iv+34+6 plates. (Washington, DC: Government Printing Office, 1972.) [229]

US Department of Agriculture: Agricultural Research Service. Marketing Research Report No. 482: Hypobaric Storage of Mature-Green Tomatoes. By W. E. Tolle. Pp. 9. (Washington, DC: Government Printing Office, 1969.) 15 cents. [229]

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